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CORRIGENDA.

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PROCEEDINGS

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AMERICAN PHILOSOPHICAL SOCIETY

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FOR PROMOTING USEFUL KNOWLEDGE

A POSSIBLE EXPLANATION OF UPPER EOCENE CLIMATES.

By EDWARD W. BERRY.

(Read April 21, 1922.)

No single problem has awakened more interest among geologists and botanists than the climatic significance of the fossil floras discovered in Arctic lands. This interest, although losing some of the zest of novelty, has remained unabated since the first announcements by Professor Heer nearly two generations ago, down to the present. A great variety of hypotheses have been advanced to explain their apparent anomalous distribution. These range all the way from Neumayr's naïve suggestion that organisms have completely changed their environmental requirements during the ages to the thesis recently advanced by Knowlton[1] that Cretaceous and Tertiary climates, as well as those of earlier geologic periods, were controlled by earth heat, and were not subject to solar control, as they are at the present time.

Everyone will, I think, admit that the faunal and floral evidence throughout the major part of geologic time, in so far as it is known, indicates a greater uniformity of climatic conditions and less contrast between high and low latitudes than exists at the present time. There are few, however, who will deny that there were contrasts at all times between high and low latitudes. Throughout most of known geologic time climatic zones appear less marked than now, and I believe that we can rely on the validity of appearances on this point. There are, however, several times in the past when climatic zones were sharply marked, and all of these were at times of land extension and sea

[1] Knowlton, F. H., Geol. Soc. Amer., *Bull.*, Vol. 30, pp. 499–566, 1920.

restriction, as shown by the recurrent periods of glaciation, beginning in pre-Cambrian time, of which that of the lower Permian was more extensive than that of the familiar Pleistocene.

Climatic zones may also have been marked during the times of land emergence, when marine deposits were largely withdrawn from the area of the present land surface of the globe, which events have in general furnished the basis for what are regarded as the systemic rock and time boundaries of geology. If this possibility could be proved it would help to explain the numerous examples of extinction and evolution that emphasize the geologic time table, and it would obviously leave few available marks in the record of life or in the sediments now available for study.

I have always been intensely interested in this subject, as who has not? Circumstances have, however, kept me rather fully occupied with the Mesozoic and Cenozoic floras of lower latitudes. Recently, in working up some Eocene floras for the Geological Survey of Canada—a subject which I approached while fresh from work on the middle and upper Eocene floras of southeastern North America—I was much impressed with the total dissimilarity between these Canadian floras, which are a part of the so-called Miocene[2] Arctic flora of Alaska, Greenland, Iceland, Spitzbergen, etc., and the contemporaneous flora of our Gulf states. This led to a general survey of the subject, some of the results of which are presented in the following notes.

It may be mentioned parenthetically that both paleozoölogy and paleobotany have suffered the drawbacks incident to the fact that their chief cultivators have been resident in the North Temperate Zone. I do not recall a single paleobotanist who has had a first-hand acquaintance with the tropical floras of the present. The same statement is to a very great extent true of paleozoölogists, and I gravely question whether those who cultivate the field of invertebrate paleontology concern themselves greatly with the results of recent researches in oceanography and their bearing on problems of distribution in the past.

After seeing the sub-tropical existing flora of southern Florida, the tropical flora of the Antilles and Central America, and the tropi-

[2] Generally recognized to be of upper Eocene age in recent years.

cal, sub-tropical, and temperate rain forest floras of South America, I am convinced that no known fossil flora of Cretaceous or Tertiary age in the United States can properly be called tropical in any but a most loose and uncritical use of that term. Much less can it be applied to the Arctic Tertiary floras. It is true that many of these fossil floras indicate warmer climatic conditions than prevail at the present time in the same latitudes, and it is also true that many of their elements have a seemingly unusual latitudinal range.

Most temperate rain forests of the present would have been pronounced "tropical" by paleobotanists if they occurred as fossils, and I have repeatedly called attention to the resemblance of our Atlantic Coastal Plain Upper Cretaceous floras to temperate rain forests, in which the same mingling of types is observed.

Most of the familiar plants that are enumerated in fossil floras as being of especial, that is, tropical climatic significance, belong to large genera whose species have a wide range and have become adapted to a variety of habitats. Take, for example, palms, which, in my experience in the Yungas of Bolivia, stick closely to the tropical and sub-tropical altitudinal zones; and we find some modern species, incidentally very similar to a great many fossil species, ranging northward as far as North Carolina, and southward as far as Valparaiso, Chile. Another type frequently mentioned in fossil floras is the cinnamon or camphor tree—the two most romantic names applied to two of the many existing species of the genus *Cinnamomum*. The modern species range well into the temperate zone; in fact, the commercial supply of camphor comes largely from Formosa and Japan, the tree being hardy as far north as about Latitude 35° in the latter country with its oceanic climate. Introduced into Florida, *Cinnamomum* has been seeded pretty widely by birds, and is perfectly hardy as far north as Tallahassee in that state.

Tree ferns constitute a third item in the paleobotanist's tropical repertoire, although the most magnificent modern tree ferns are found in temperate rain forests, such as those of New Zealand, on high peaks in the tropics (above the tropical altitudinal zone), or in the montaña valleys of the Eastern Andès (above the tropical altitudinal zone). The bread fruit is another spectacular fossil, and although

the modern species in nature appear to be confined to tropical and sub-tropical environments, fossil forms are found in association with temperate types, as, for example, in the Upper Cretaceous of Greenland, the early Eocene of the Rocky Mountain region and Gulf states, and the lower Miocene of Florida (it is obvious that the advocates of former torridity can exactly reverse the bread-fruit argument), so that one must use considerable caution in any attempt to interpret its meaning in terms of climate.

It may be remarked parenthetically that I do not consider Aphlebiæ as indicative of heat, but humidity. That *Ginkgo* is not a tropical type, but appears to be hardy throughout the Temperate Zone, and does not flourish under cultivation in the Tropics. That Cycads and Conifers are not good criteria for either moist or sub-tropical climates, but quite the reverse. That *Gleichenia* and its present-day segregates, although commonly found in the Equatorial Zone at the present time, are not limited to the tropical part of that zone, but are frequently more at home in the sub-tropical or temperate altitudinal zones in Equatorial uplands, as in Hawaii, the Eastern Andes, etc.

Doubtless terrestrial plants are better indices of climate than are other organisms, and they are admittedly more important in this respect than marine forms of life. Plant fossils have this merit aside from any question of botanical identification, and this feature seems to have been lost sight of by numerous critics of paleobotanical practise: that the size and form of leaves, their texture, the arrangement and character of their stomata, and the seasonal changes in wood, afford criteria that are quite as valuable climatically even though the species or genus to which they belong remains undetermined. Without venturing further on the sea of words that constitutes the elusive generalities of most discussions of past climates, I propose to contrast the Tertiary "Arctic flora" with that which existed contemporaneously in lower latitudes, after which I will suggest a possible explanation to account for the observed facts.

The exact age of this "Arctic flora" can not be conclusively proven, but it is a reasonable assumption that it is of approximately the same age wherever found, and this assumption rests on actual community of composition, and not on an environmental premise,

although there is also something to be said for the latter. Heer studied these Arctic floras after his monumental work on the Tertiary floras of Switzerland, and he called them Miocene, a fashion that still persists in some quarters. It is lost sight of that Heer was prone to see his familiar Swiss Tertiary species in what were often very imperfect fragments from the far North; and it is also true that Heer recognized no Oligocene period, but included the fossils of this age in the Miocene, which, to that extent, never meant more than "old Miocene"—that is to say, Oligocene.

The "Arctic Miocene" flora is certainly younger than that of our Fort Union of the western United States and Canada, whose facies continues into the Wasatch of the same region. It is overlain in places by marine Miocene strata, and interbedded with upper Eocene marine faunas, as at Herendeen Bay, Alaska. I have been inclined to consider it as also younger than the Green River flora of the western United States, and to be of approximately the same age as the Jackson flora of the southern Atlantic Coastal Plain. It is certainly older than any known lower Miocene flora of the United States or Europe, and the following comparisons do not suffer any diminution of conclusiveness, if the Arctic flora should eventually be proven to be slightly older or slightly younger than the Jackson, for we now know as considerable floras in the southern Coastal Plain from the immediately antecedent Claiborne group, and from the immediately subsequent Oligocene (Catahoula and Vicksburg), and all of these have substantially the same facies and climatic significance as has the flora of the Jackson group.

The Jackson flora, a detailed account of which is in press as Professional Paper 92 of the United States Geological Survey, contains considerably over one hundred species. These represent genera such as *Acrostichum, Pistia, Canna, Thrinax, Phœnicites, Engelhardtia, Momisia, Ficus, Coccolobis, Pisonia, Myristica, Anona, Inga, Cassia, Bauhinia, Sophora, Lonchocarpus, Fagara, Cedrela, Banisteria, Burserites, Cupanites, Dodonæa, Grewiopsis, Bombacites, Ternstrœmites, Cinnamomum, Mespilodaphne, Nectandra, Rhizophora, Terminalia, Conocarpus, Conbretum, Myrcia, Calocarpum,* etc. There is not a distinctly temperate type among them and this flora comes as near meriting the term "tropical" as any fossil flora known from the

Temperate Zone. I regard it, in fact, as sub-tropical. Space does not permit a discussion of the range of the existing representatives of this Jackson flora. This is done in the publication cited above, and the generic names mentioned will serve those of my botanical readers who wish to check my conclusions. Now, let me compare this Jackson flora with the so-called Kenai flora of Alaska.[3]

The Kenai flora, as published, consists of about 120 species. The most abundant forms are willows, oaks, poplars, walnuts, beeches, birches, hazels, and alders—distinctly Temperate, and cool rather than warm Temperate types. Perhaps the most abundant plants individually, certainly the widest ranging geographically in northern latitudes (Holarctica), are the leaves of hazel bushes (*Corylus*). Of the 54 genera of Knowlton's list, the following nine are not present in the existing flora of North America: *Ginkgo, Glyptostrobus, Taxites, Hedera, Paliurus, Elæodendron, Pterospermites, Trapa,* and *Zizyphus.*

It may seem that I am juggling the evidence in omitting these nine genera from further consideration, but let me point out that the three of these about which there seems to be no doubt regarding their identity, namely, *Ginkgo, Trapa,* and *Glyptostrobus,* are all Temperate types in the existing flora. The remaining six genera are under more or less suspicion of quite a different order from any differences of opinion among paleobotanists regarding the identification of the hazels, birches, alders, etc., with which they are associated. Opinion might differ as to whether a particular species of the latter was a *Betula* or *Alnus,* an *Ulmus* or a *Carpinus,* or a *Planera;* or whether one or several species of *Corylus* should be recognized as distinct species; but opinion is unanimous that the choice is thus narrowed, whereas in the case of such things as *Taxites*—all any one knows is that it represents some Conifer. Why waste time trying to explain the climatic significance of *Paliurus,* a mostly extinct genus, when the particular fossil is probably not a *Paliurus;* or why concern oneself with an Arctic species of *Zizyphus* when the form in question is probably a *Ceanothus?* I ask, can any one prove that the form-genus *Ptero-*

[3] As listed by Knowlton in 1919. Hollick's extended labors on this flora which may be expected to contain some new species, will undoubtedly modify, but will not alter the force of the present comparison.

spermites is genetically related to the existing genus *Pterospermum?* or that *Elæodendron* is a sound botanical identification? I think not!

On the other hand, the great mass of not only the Kenai but of all the Arctic Tertiary floras are the readily recognizable, normal units of a natural assemblage, which individually leave but slight room for differences of opinion regarding their identity. If fruits chance to be found in association with the leaves, they are such things as birch or alder cones, never the fruits of the "suspects" above mentioned.

Of the remaining genera listed in the Kenai flora, all but the following six are represented in the existing flora of Canada: *Æsculus, Diospyros, Ficus, Liquidambar, Sequoia*, and *Taxodium*. It may be said of these that the *Æsculus* may not be an *Æsculus*, but a *Hicoria;* that the two species that have been referred to *Ficus* do not belong in that genus; and that *Sequoia* is on the verge of extinction at the present time and its modern range bears little relation to its former range. The case of *Sequoia* is of especial interest in its bearing on my thesis. Formerly a *Holarctic* type, it survives today in a most restricted area particularly favored by humidity.

The remaining genera of the Kenai flora appear to be determined with reasonable certainty. Not only are 39 of these represented in the existing flora of Canada, but the following are still represented in the existing flora of Alaska, or adjacent areas in northwestern Canada, or as far north as Labrador and Hudson Bay in eastern Canada: *Abies, Acer, Alnus, Alnites, Andromeda, Betula, Carex, Corylus, Equisetum, Fraxinus, Myrica, Osmunda, Phragmites* (grass), *Picea, Pinus, Populus, Prunus, Pteris, Quercus, Sagittaria, Salix, Spiræa, Thuites*, and *Vaccinium*.

Seventeen of the Kenai species are Conifers, and the only types that would seemingly be out of place in a cool-Temperate climate with well-distributed moisture are *Liquidambar, Paliurus, Taxodium*, and *Zizyphus*. I have already given reasons for discrediting the determinations of some of these, and all of them have frequently been found fossil in Temperate assemblages.

The most diversified Arctic Tertiary flora known is that described by Heer from western Greenland. As considerably over-elaborated by Heer it comprises 282 species. Of this number there are 8 worth-

less objects described as fungi; there are 20 ferns; 28 conifers; 21 monocotyledons, of which most are fragments of grass- or sedge-like leaves, generically and specifically undeterminable. Among the monocotyledons are two nominal species of *Flabellaria*—a form-genus proposed for undeterminable palm fossils. Neither of these is very convincing, and if they are correctly figured by Heer I would never think of calling them palms, and yet every textbook speaks of the Tertiary palms of Greenland, although they always have been purely subjective.

The bulk of the Greenland Eocene flora consists of dicotyledonous leaves. Among these we find *Ficus*, but this does not mean that figs were members of this flora. The determination of *Ficus* on Heer's part was only tentative, and he published it with a question mark behind it; moreover, it does not suggest any of the numerous species of *Ficus*, either fossil or recent, with which I am familiar. Among the Greenland Tertiary dicotyledons the following genera predominate: *Populus, Salix, Alnus, Betula, Corylus, Fagus, Quercus, Juglans, Acer, Laurus, Andromeda, Fraxinus, Viburnum, Cornus, Magnolia, Ilex, Celastrus, Rhamnus, Rhus*, and *Cratægus*.

These are all genera that range for greater or less distances into, and some, such as *Populus, Salix, Alnus*, and *Betula*, range entirely across the cool-Temperate Zone. Any one is, of course, at liberty to call this Greenland Tertiary flora "tropical"—after the most detailed comparison and discussion its original describer did not so consider it, nor do I.

In nearly every case where a Greenland Eocene genus is indicative of conditions warmer than cool-Temperate, as, for example, the genera *Pterospermites, Sapindus, Zizyphus, Colutea, Laurus*, and *Dalbergia*, the botanical determinations can, and are, seriously questioned, and all that they can be asserted to mean is that the respective forms are similar to forms from other regions which some one, generally Heer himself, called *Colutea, Dalbergia*, etc.

The most northerly known Eocene flora is that described by Heer in the fifth volume of his "Flora Fossilis Arctica" from near Cape Murchison in Grinnell Land. As elaborated by Heer this flora comprised 30 species, but it may well be pointed out that a number of these have no existence outside the literature of paleobotany. Thus

there is a single *Equisetum* instead of two species; the four species of *Feildenia* represent but a single botanical species, of still unknown botanical affinity; the five pines represent a less number of species, and were named by Heer before the old genus *Pinus* was segregated; the two *Phragmites* represent leaf fragments of grasses or sedges, and nothing more definite, and the *Caulinites* is a sedge or grass rootstock, while other leaf fragments are named *Carex*. *Iridium* and *Salix* represent absolutely nothing determinable, as Heer practically admits in his discussion of them; the two species of *Corylus* described represent but a single species, to which what Heer called *Alnus* should also probably be referred; the *Ulmus* is also a *Corylus*, in my opinion; the *Viburnum* is a *Populus;* and the *Tilia* is a *Corylus*. These suggested changes may be merely a matter of opinion, they certainly are my opinions, but the suggested revision is no essential part of my argument.

Revised as suggested in the preceding paragraph, the result is: *Equisetum, Feildenia, Thuites, Taxodium, Pinus, Abies,* sedge or grass fragments, *Populus, Betula,* a variety of *Corylus* leaves, and apparently a fragment of a *Nymphæa* rootstock. Considered in this attenuated form this flora is still remarkable enough. The flora in the immediate vicinity of Cape Murchison under the climatic conditions of the present includes *Carex,* various grasses, and the genera *Salix* and *Vaccinium*. The present isotherms would have to swing 15 to 20 degrees northward to permit the existence of such an Eocene flora as that listed above in Grinnell Land. The dwarf birch of the present reaches the latitude of Grinnell Land in Spitzbergen; and white birches occur north of the Arctic Circle in Europe, Asia, and North America, or within about ten degrees of the Grinnell Land fossil flora.

The significant feature about these Eocene Arctic floras is that they show a comparable northward swing of not alone their northern limits, but also of their southern limits, which in turn is comparable to the northward advance of the Jackson flora that I have considered to be of the same age. The Jackson flora reaches Latitude 37° North. The most similar existing flora to that of the Jackson does not extend above Latitude 26° North, and then only under especially

favorable conditions of situation with respect to warm ocean currents. This is a difference of 11 degrees. The flora of the Jackson was, moreover, a coastal flora, and I have not the slightest doubt but that had the Mississippi embayment extended five degrees farther North, its shores would have been clothed with the same Jackson flora, for at that time similar floras are found in the Paris Basin in Latitude 49° North, in southern England in Latitude 51° North, and along the expanded Mediterranean sea of the Old World.

The southern limit of the contemporaneous "Arctic flora" is about Latitude 45° North in North America (British Columbia), and about 57° North in Europe (Isle of Mull). It seems to me that the essential concordance of these facts is significant, and whatever may be thought of them, it would certainly seem to be difficult for any one to claim that these various Eocene floras mentioned do not show a climatic change in passing northward from the equator toward the pole. Moreover, at present—a time of, in many ways, an abnormal climate in a geologic sense; with rather sharp zoning, although not nearly so sharp as the textbooks would have us believe; a time of almost, if not quite, unprecedented land expansion in the Northern Hemisphere—which I believe expresses a causal relationship; the reliable members of these Eocene Arctic floras range much farther southward than they did in late Eocene time.

That changes in the geographic distribution of land and water might prove sufficient to account for these Eocene Arctic floras is suggested by the climatic influence of the northward drift of warm water in the North Atlantic at the present time; which influence in western Europe and Spitzbergen is a trite fact, familiar to all. If the reader will consult an Isothermal map, and will compare the North Atlantic region with that of Behring Strait, or with the center of Siberia (the latter the present location of the cold pole), the combined influence of the northward drift of warm oceanic water from lower latitudes, and the influence of the relative amounts and geographic position of lands and seas, is seen to be most astonishing.

For example, the January isotherm of —25° C., which is at Behring Strait, is 18 degrees farther North in Baffin Bay, and reaches to the pole itself North of Spitzbergen. The July isotherm of 5° C. (41° F.), which is at Behring Strait, swings 10 degrees farther North

in Baffin Bay, and 15 degrees farther North in western Spitzbergen, and there is a corresponding northward swing over the ocean between Spitzbergen and Nova Zembla.

When the times of widest extension of equable and mild climates in the past are considered—the most conspicuous of which, in post-

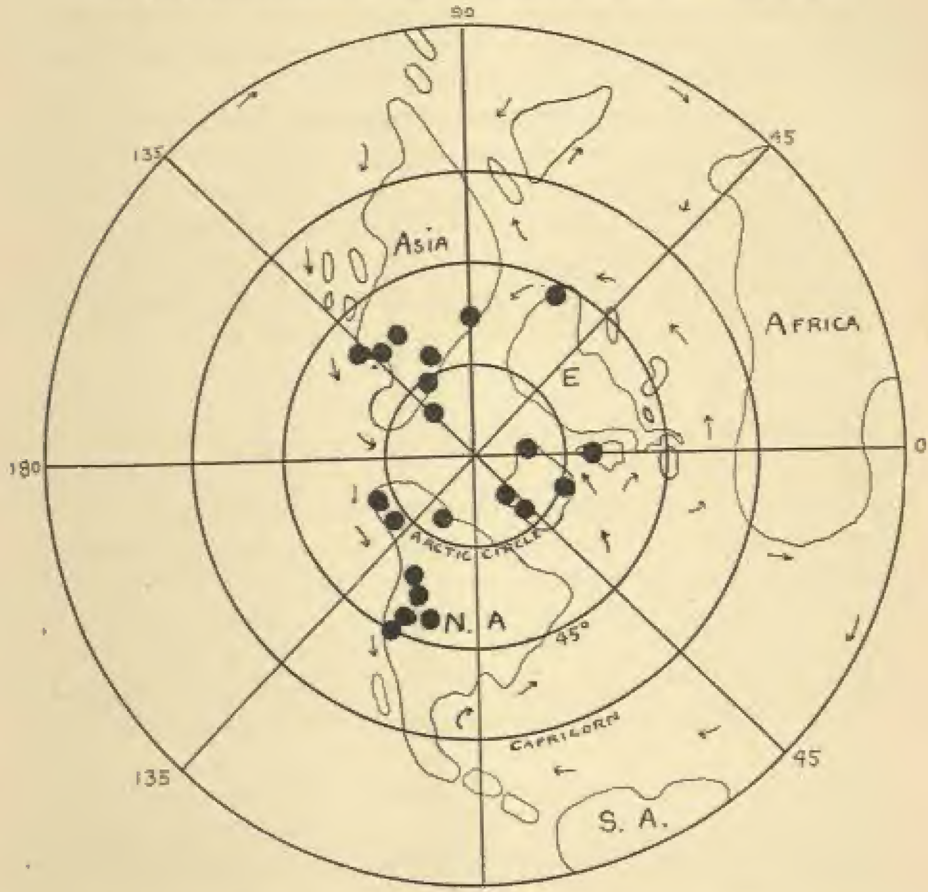


FIG. 1. Sketch map of the middle Eocene showing location of the "Arctic floras" of the upper Eocene.

Paleozoic times, are those of the Jurassic, Cretaceous, and later Eocene—they all appear to bear a definite relationship to periods of sea extension and reduction of land areas. To give objectivity to this statement I have prepared a map, on a North Polar projection,

showing the supposed distribution of land and water in mid-Eocene time.[4]

It will be noted first that the land areas are greatly reduced, amounting to perhaps as much as 40 per cent. of the present land area of the Northern Hemisphere; and their place is taken by mostly shallow seas in low latitudes. The Mediterranean regions of the world are almost continuously under water, and these have free polar connections from the Atlantic, Pacific, and Indian oceans, as well as from the greatly expanded *Tethys*. If the reader will now consult Sir John Murray's quantitative estimates of the present influence of the Gulf stream on western Europe, it will be obvious that a distribution of land and water such as I have indicated for mid-Eocene time would be most important in climatic results. These effects would be cumulative, and, in my opinion, a sufficient cause to account for the observed distribution of the fossil floras of the upper Eocene. That it is a matter of distribution of land and water plus oceanic circulation is indicated by the more northern Eocene distribution of plant types of low latitudes in Europe than in North America, as at the present time.

I would imagine a certain lag in observable effects, due to this cumulative action, and the progressive reduction of polar ice, and the ultimate nullification of appreciable cold ocean currents. I have indicated on the map, in a most general way, the probable directions of the oceanic circulation, as well as the localities where Eocene plants have been found in high latitudes. No one knows exactly what paths a given geographic pattern would impose on definite ocean currents, there being so many factors involved; but certainly no one can object to the statement that the general, or what might be termed the planetary circulation would dominate, except as modified by subordinate and unvaluated factors; or that the northward drift of warm surface waters would be operative.

It will be noted that all of the most northern Eocene plant localities are in coastal situations, favorably situated to receive the full

[4] This map makes no pretense to accuracy, which is largely a matter of opinion, nor is there anything original about it. It is essentially the map compiled from De Lapparent's "Traité" by W. D. Matthew, and used by H. F. Osborn in his "Age of Mammals."

benefit of this drift of warm oceanic waters, whereas the most southern localities, which here mark the upper Eocene southern limit of alders, birches, hazels, etc., are found at about Latitude 45° North in North America, and fairly well away from coasts in a region where it is reasonable to assume a continental climate may have prevailed, especially if there were mountains intervening, as the textbooks would have us believe.

None of these most southern Eocene floras of the cool-Temperate type come within many degrees of reaching the existing southern limits of their well-determined genera. For example, the hazel (*Corylus*) has its present southern limit at Latitude 31° North, whereas in Eocene time this limit was at about Latitude 45° North. At the time these Temperate types were extending their range northward, almost to the pole itself, warm temperate or sub-tropical types had invaded our Gulf States as well as southern and central Europe.

The conclusion seems probable that the whole world had, at that time, a more or less mild and equable climate, prevailingly of the oceanic type, and that the primary cause of this oceanic climate was the diminished and low-lying land areas, and the increased sea areas; so arranged as to permit a maximum of circulation between equatorial and polar regions. The floras show that in spite of this relatively mild and equable climate, then as now, the polar regions were considerably cooler than the equatorial regions. At the present time, because of the great expanse of the Pacific ocean in the equatorial region, its average surface temperature is 19.1° C., as compared with 16.9° C. for the Atlantic and 17° C. for the Indian ocean.

The consensus of opinion that the land masses of the Northern Hemisphere were the main theater of evolution of late Mesozoic and Tertiary terrestrial life, both animal and vegetable, may seem to be opposed to such a free oceanic circulation between the equatorial and polar regions as I have indicated, but this is only an apparent and not a real difficulty. The land emergence whose culmination furnishes geologists with the basis for a boundary between Mesozoic and Cenozoic afforded abundant land paths for the dispersal of terrestrial animals and plants, as witness the essential community of the faunas and floras of early Eocene time throughout the whole of Holarctica.

This time of land connections was followed by sea transgression and land shrinkage, especially pronounced geographically in the middle Eocene (Claiborne-Jackson of the Atlantic Coastal Plain; Lutetian-Auversian of the French chronology).

During this last period, according to Osborn, the mammalia undergo independent evolution without intercontinental dispersal, and are sharply marked into Palæarctic and Nearctic realms, that are entirely extinct. In Oligocene time land bridges were renewed with community of terrestrial faunas, and not since that time of renewal of intercontinental interchange of species have there been as free water connections between the equatorial and Arctic regions as there were in middle Eocene times. The geographic pattern has fluctuated, to be sure, but as a whole the geography has more nearly approximated that of the present, and this, I believe, was the most important factor in bringing about the climatic facies of the late Tertiary faunas and floras. Nowhere in the North Temperate Zone, as far as I recall, do we find terrestrial faunas or floras, or marine littoral faunas, indicative of as warm conditions in the middle and upper Miocene, or Pliocene, as do those of the later Eocene, and perhaps also those of the early Oligocene.

It is not impossible that some less invoked factor, such as this distribution and attitude of the land and sea, may even account for glacial periods, as Sir Charles Lyell suggested long ago, rather than that these have been the result of causes that are purely speculative, such as sun spots, carbon dioxide, dust, reversal of oceanic circulation, etc. I do not deny that these latter may not have been factors, even major ones, but at the present time they belong, in my judgment, in the same category with that hypothesis which predicates a shifting of the poles.

I fully realize that the facts presented in the foregoing notes by no means solve the difficulties that arise in our endeavors to explain the distribution of organisms in the past, but that they furnish one considerable factor in the uniformitarian interpretation of early Tertiary floral distribution, is my reason for calling attention to the subject in the present brief way.

JOHNS HOPKINS UNIVERSITY,
April, 1922.

THE EARTH INDUCTOR COMPASS.[1]

By PAUL R. HEYL and LYMAN J. BRIGGS.

(*Read April 22, 1922.*)

The instrument and accessories described in this memoir have been developed with especial reference to use in vessels for the navigation of the air. Navigating conditions in aircraft are such that little or no reliance can be placed on the indications of the ordinary magnetic compass. For this there are two principal reasons.

Lack of space in airplanes forces the pilot and his instrument board to be located in a region of considerable magnetic disturbance, due to the proximity of the engine and other magnetic bodies. Occasionally these are variable, such as the steering rod, which in some planes is of steel. Compensation, after the manner familiar to navigators of the water, is not always a satisfactory solution of the difficulty. Apart from the variable disturbing elements above referred to, the magnetism of the engine may change considerably during a long flight. On the U. S. Army airway from Dayton, Ohio, to Washington, D. C., it has happened that the compass has developed an error of as much as forty degrees before the trip was completed. In the endeavor to eliminate such troubles the compass is sometimes placed in the rear portion of the plane, and readings taken by devices more or less complicated, as in the German Bamberg compass. Such a plan, however, does not eliminate the second and more serious cause of disturbances.

The accelerations of an airplane are greatly in excess of those to be found in a water vessel; and the directive force of the earth's field on the magnetic needle is weak. Due to the great acceleration, the weak directive force, and the necessary inertia of the needle and card, the magnetic compass possesses rather a long memory for disturb-

[1] A memoir to which the Magellanic Premium was awarded January 6, 1922, by the American Philosophical Society.

ances to which it has been subjected. It is not difficult to give the plane such a motion as to set the compass card spinning on its pivot so that before its motion subsides sufficiently to allow of even an approximate reading the plane has traveled two or three miles. The great speed of the plane (ordinarily from seventy to one hundred miles an hour) and the comparatively sharp and sudden turns sometimes executed produce a set of conditions of an order entirely different from those to which a navigator of the water is accustomed. The attempt to meet these disturbances by damping the compass card is not a satisfactory solution; for damping, while it diminishes disturbance, also decreases sensitivity, none too great at best.

The difficulty of the situation is perhaps best shown by the fact that the Great War, which produced, under stress of necessity, so many inventions, closed without having brought out any satisfactory form of airplane compass on either side of the conflict.

For satisfactory service under conditions of this nature the earth inductor in connection with a galvanometer possesses a fundamental advantage over the magnetic needle. Unlike the latter, it has no memory. From instant to instant it furnishes an electromotive force determined by its orientation with respect to the earth's field, irrespective of its past or present state of translatory motion. This advantage has not been unrecognized by previous workers (Dunoyer: British Patent 4609 of 1907; Chabot: British Patent 9912 of 1903; Bliss: U. S. Patent 1,047,157 of Dec. 17, 1912). It may be noted that no one of these proposed devices possessed sufficient practicability to bring it into use during the war.

In all previous attempts at the construction of a compass of this type, the current developed in the rotating coil, amplified if necessary, was caused to pass through a galvanometer, and the course of the vessel indicated by the amount of deflection produced. The instrument described in the present memoir differs from all previous attempts in the following respects:

1. It employs a null method for its indications, and therefore enjoys all the advantages of sensitivity characteristic of null methods as a class. As long as the vessel lies in the course predetermined by the pilot, no deflection is produced in the galvanometer.

2. A course-setting device of a novel type is employed. By turning a movable dial carrying compass graduations to the desired course-mark, the electrical connection of the galvanometer to the earth inductor is so arranged that the galvanometer will read zero only when the vessel is in the desired line. This device enables the pilot to control a compass situated at a safe distance from magnetic disturbances without the use of a moving mechanical connection.

3. This course-setting device possesses a feature which enables the pilot to distinguish between north and south, or, in general, between the two opposite directions which the vessel may take in any line.

4. A method is provided for eliminating the errors due to rolling and pitching, arising from the vertical component of the earth's field.

5. By the judicious use of iron in the core of the coil, the size of the earth inductor may be greatly reduced and the current output increased without the introduction of any sensible error.

In the course of its development this instrument has naturally passed through several forms. Only the final form will here be described.

Current is generated by the rotation in the earth's horizontal field of an armature rotating about a vertical axis. A commutator and four collecting brushes, spaced at 90°, take off current from the armature. For simplicity we may first suppose the vessel to lie in the magnetic meridian. The brush system as a whole may then be turned to such a position that one pair of brushes furnishes no electromotive force and the other pair a maximum. The brush system may now be fixed in this position with respect to the vessel. If now the vessel be turned through an angle θ from the meridian, the two pairs of brushes will furnish electromotive forces proportional respectively to $\sin \theta$ and $\cos \theta$. In this it is assumed that θ is measured positively in a clockwise direction from the meridian, and that the commutator connections are so made that the algebraic sign of the voltage at each pair of brushes will be that proper for the sine or cosine of the angle in whatever quadrant that angle may be located. For this reason the two pairs of brushes will be henceforth referred to as the sine and cosine brushes, respectively.

If the sine brushes only are connected to the galvanometer, its reading will be zero only when the vessel lies in the magnetic meridian; and if the cosine brushes only are connected, the reading will be zero only in a magnetic east and west line.

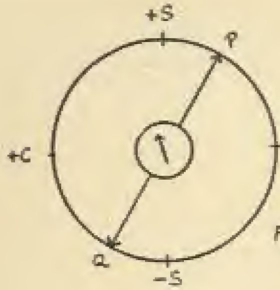


Fig. 1.

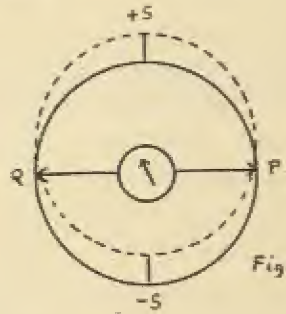


Fig. 2.

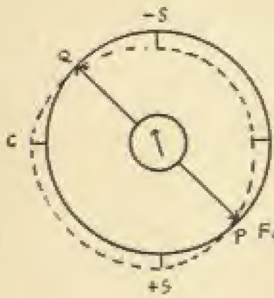


Fig. 3.

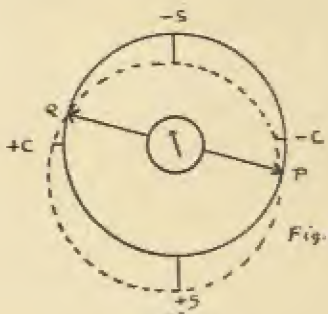


Fig. 4.

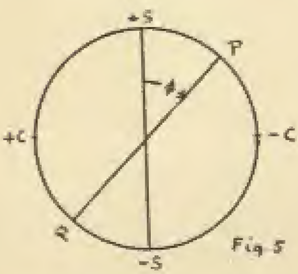


Fig. 5.

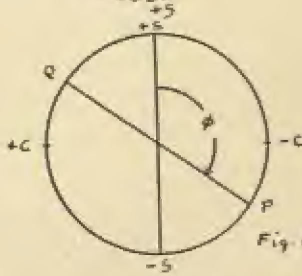


Fig. 6.

We may connect the two pairs of brushes in series so that the voltage applied to the galvanometer is $\sin\theta + \cos\theta$. This function will be zero only when $\theta = 135^\circ$ or -45°; that is, when the vessel lies in a northwest and southeast line. And if we use the combination $\sin\theta - \cos\theta$, this will be zero when $\theta = 45^\circ$ or 225°; that is, in a northeast and southwest line.

In general, since

$$m \sin \theta + n \cos \theta = 0$$

if $\tan \theta = -(n/m)$, the galvanometer reading may be made zero in

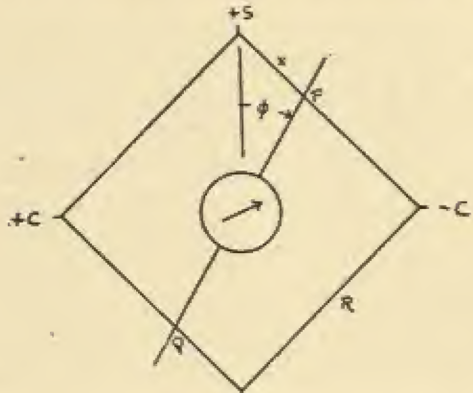


Fig. 7

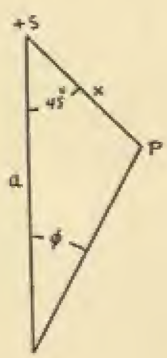


Fig. 8

any desired direction by combining, additively or subtractively, suitable fractions or multiples of the voltages from the two pairs of brushes. This is the underlying principle of the course-setting device

known as the dial switchboard, which is an important part of this invention.

Such a switchboard may assume various forms. The form adopted in practice is very simple in actual construction, although not so simple in mathematical theory as certain more complicated practical forms that preceded it in the course of its development. Its theory is illustrated in Figs. 1–8.

In Fig. 1 we have a closed circuit of resistance, the sine and cosine voltages being connected to the circle at four equally spaced points $\pm S$, $\pm C$. The galvanometer connections are made at any two diametrically opposite points P, Q, of the circle. Indicating, as in Fig. 2, positive potential by a line drawn radially outward from the resistance circle, and negative potential by a line drawn inward, and supposing the vessel to lie in an east and west line, so that the sine voltage is a maximum and the cosine voltage zero, we have a distribution of potential around the circle indicated by the dotted line. The only diametrically opposite points which will be at equal potentials are those at the ends of the horizontal diameter. This, then, will be the null position for an east and west course.

If, on the other hand, the sine voltage is zero and the cosine voltage a maximum, as is the case in a north-south course, the null position will be that of the vertical diameter.

If the vessel lies in a northwest and southeast line, $\theta = -45°$ (or $135°$), sin θ is negative and cos θ positive, both, however, being equal in absolute numerical value. Maintaining the connections shown in Fig. 1, but changing the sign of S, the distribution of potential will be as shown in Fig. 3. Points $+S$ and $+C$ are at equal potentials; between them we have a level of potential, and no current flows in this quadrant. The same is true of the quadrant between $-S$ and $-C$. But between $+S$ and $-C$ (and $+C$ and $-S$) a fall of potential exists. Symmetry indicates that the only equipotential points for the galvanometer leads lie on a line making $-45°$ with the vertical diameter; that is, pointing northwest with reference to the cardinal points previously determined (Fig. 2).

Let the vessel now veer a little farther to the west; the sine voltage will increase (negatively) and the cosine voltage decrease (posi-

tively). Fig. 4 represents the distribution of potential in this case. The potentials at $+S$ and $+C$ are no longer equal, that at $+S$ being greater than that at $+C$. In consequence, the line of contact will be shifted, as it should be, a little nearer to the horizontal than in Fig. 3.

It is a curious fact that in general the angle of the null contact line with the $\pm S$ line is not always equal to the course-angle θ of the vessel with the magnetic meridian. There is, as we have just seen, perfect correspondence at the cardinal and 45° points; but for points within each octant, as in Fig. 4, there is a departure rising to a maximum of about 4° near, but not at, the center of each octant, as the general mathematical theory will show.

Assume the S and C voltages applied as in Fig. 5 (same as Fig. 1) and let the contact line of the galvanometer be inclined at an angle ϕ to the $\pm S$ line, measured positively in the clockwise direction. We shall first suppose ϕ limited to the first quadrant.

$$\text{Potential at } P = \left(\frac{90^\circ - \phi}{90^\circ}\right)\sin\theta - \left(\frac{\phi}{90^\circ}\right)\cos\theta,$$

$$\text{Potential at } Q = -\left(\frac{90^\circ - \phi}{90^\circ}\right)\sin\theta + \left(\frac{\phi}{90^\circ}\right)\cos\theta.$$

If these are equal, we must have

$$\tan\theta = \frac{\phi}{90^\circ - \phi}. \tag{1}$$

Since equation (1) is algebraic in ϕ and transcendental in θ, it is evident that no linear relation can exist between ϕ and θ. Solving for ϕ we have

$$\phi = \frac{\pi}{2}\frac{\tan\theta}{1 + \tan\theta} = \frac{\pi}{2}\frac{\sin\theta}{\sin\theta + \cos\theta}. \tag{2}$$

Now

$$\phi - \theta = \frac{\pi}{2}\frac{\sin\theta}{\sin\theta + \cos\theta} - \theta, \tag{3}$$

which will be a maximum or minimum where

$$\frac{d(\phi - \theta)}{d\theta} = \frac{\pi}{2}\frac{1}{(\sin\theta + \cos\theta)^2} - 1 = 0. \tag{4}$$

The approximate first quadrant roots of (4) are 17° 24′ and 72° 36′ instead of the mid-points of the octants 22° 30′ and 67° 30′. At these roots the value of $\phi - \theta$ is nearly 4°, as shown in the following table.

θ.	ϕ.	$\phi - \theta$.
17° 24′	21° 28′	4° 4′
22° 30′	26° 22′	3° 52′
45°	45°	0
67° 30′	63° 38′	−3° 52′
72° 36′	68° 32′	−4° 4′
90°	90°	0

If ϕ lies in the second quadrant (Fig. 6), we have

$$\text{Potential at } P = -\left(\frac{\phi - 90^\circ}{90^\circ}\right)\sin\theta - \left(\frac{180^\circ - \phi}{90^\circ}\right)\cos\theta,$$

$$\text{Potential at } Q = \left(\frac{\phi - 90^\circ}{90^\circ}\right)\sin\theta + \left(\frac{180^\circ - \phi}{90^\circ}\right)\cos\theta.$$

If these are equal, we have

$$\tan\theta = -\frac{180^\circ - \phi}{\phi - 90^\circ}$$

$$\phi = \frac{\pi}{2}\frac{2 - \tan\theta}{1 - \tan\theta} = \frac{\pi}{2}\frac{2\cos\theta - \sin\theta}{\cos\theta - \sin\theta}. \qquad (5)$$

The maximum and minimum values of $\phi - \theta$ in this quadrant are approximately at $\theta = 107° 24'$ and 162° 36′. The values of $\phi - \theta$ at these points are 4° 4′ as in the first quadrant.

We may now obtain by computation from (2) and (5) the values of ϕ previously indicated by symmetrical considerations.

If, as in Fig. 2, $\cos\theta = 0$ and ϕ is in the first or second quadrant, both formulas give $\phi = \pi/2$.

If, as in Fig. 3, $\sin\theta = -(\sqrt{2}/2)$, $\cos\theta = \sqrt{2}/2$ and ϕ is in the second quadrant, formula (5) gives

$$\phi = \frac{\pi}{2}\frac{3}{2} = 135^\circ.$$

To avoid in practical construction the non-uniform distribution of resistance around a circle which would be necessary to give a uni-

formly graduated dial, it is sufficient to replace the circle by a square, as indicated in Fig. 7. Contact of the galvanometer leads is made at points P and Q, whose distances from the center of the square vary with the angle of setting. It may readily be seen that the resistance included between $+S$ and P will vary more rapidly per unit angle of turn near the corner of the square than at the middle of a side.

In Fig. 8, a is a constant and x a variable side of the triangle having a constant angle 45° and a variable angle ϕ. In this triangle the points P and $+S$ correspond to the similarly lettered points in Fig. 7. In Fig. 8 we have:

$$\frac{x}{\sin\phi} = \frac{a}{\sin(135^\circ - \phi)} = \frac{a}{\sin(45^\circ + \phi)},$$

$$x = \frac{a \sin\phi}{\sin(45^\circ + \phi)}. \tag{6}$$

Now, in Fig. 7 let R be the length of one side of the square; then, if ϕ be in the first quadrant:

$$\text{Potential at } P = \left(\frac{R - x}{R}\right)\sin\theta - \left(\frac{x}{R}\right)\cos\theta,$$

$$\text{Potential at } Q = -\left(\frac{R - x}{R}\right)\sin\theta + \left(\frac{x}{R}\right)\cos\theta.$$

If these are equal,

$$\tan\theta = \frac{x}{R - x}$$

$$x = R\frac{\tan\theta}{1 + \tan\theta} = R\frac{\sin\theta}{\sin\theta + \cos\theta}. \tag{7}$$

Substituting in (7) the value of x from (6) and noting that $a = (R\sqrt{2})/2$, since R is the side of the square of which a is the semi-diagonal, we have:

$$\frac{\sqrt{2}}{2}\frac{\sin\phi}{\sin(45^\circ + \phi)} = \frac{\sin\theta}{\sin\theta + \cos\theta}. \tag{8}$$

(8) expresses, for a square frame, the relation between the course-angle θ of the vessel and the null contact angle ϕ of the dial switchboard. (8) reduces as follows:

$$\frac{\sqrt{2}}{2}\frac{\sin\phi}{\sin(45^\circ + \phi)} = \frac{\sqrt{2}}{2}\frac{\sin\phi}{\sin 45^\circ \cos\phi + \cos 45^\circ \sin\phi}$$

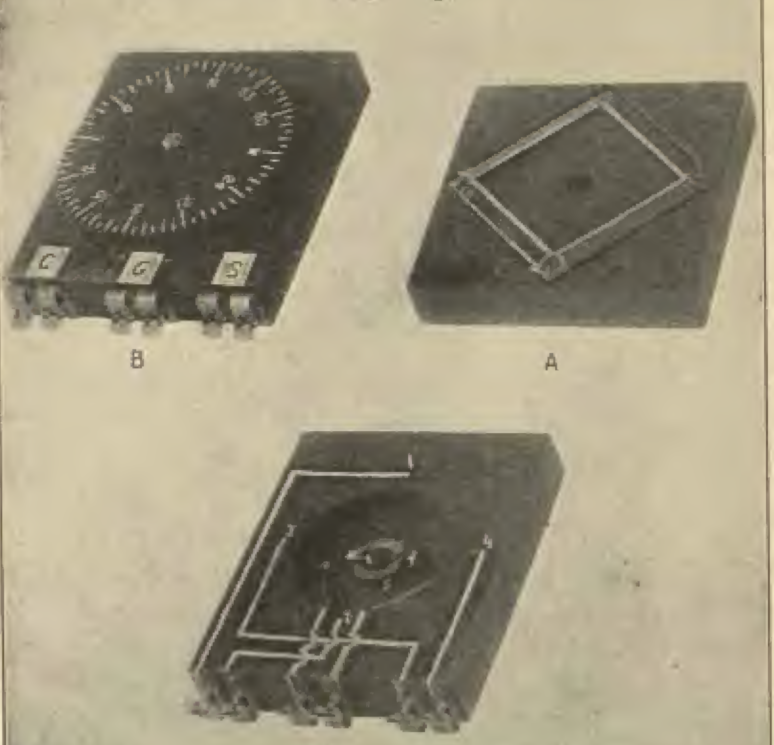
$$= \frac{\sin\phi}{\cos\phi + \sin\phi} = \frac{\sin\theta}{\sin\theta + \cos\theta}.$$

Which shows that for a square frame $\phi = \theta$, at least for the first quadrant. It may readily be shown that the same relation holds in the second quadrant as well, as indeed is indicated by conditions of symmetry.

Upon this theory may be based a very simple practical construction for the dial switchboard, illustrated in Fig. 9 (photograph).

Fig. 9*a* shows the switchboard base with dial removed, showing the square resistance frame. This frame has an inside diameter of

FIG. 9



6.5 cm. and each turn of wire is nearly 2 cm. long. It is wound with No. 30 constantan wire, and each arm has a resistance of about 37 ohms. The inner edge of the square frame stands slightly above the

outer edge, and along this inner edge the insulation is removed from the turns of wire.

Fig. 9*b* shows the assembled switchboard, front view. The dial carrying the compass divisions carries on its under side the wiping contacts which constitute the terminals of the galvanometer connections. These contacts press against the exposed portions of the wire on the square frame.

Fig. 9*c* shows the back view of the switchboard. From the four corners of the square resistance frame wires run through holes *1*, *2*, *3*, *4* to the back of the switchboard, and thence as indicated to the *S* and *C* binding posts, which in turn are connected respectively with the sine and cosine brushes. From the wiping contacts on the under side of the dial wires run through the hub *5*, which moves with the dial, to the wiping contacts *6*, *7*, from which wires run to the posts *G*, which are connected with the galvanometer.

A simple manipulation of the dial switchboard enables the pilot to distinguish between north and south, or in general between the forward and backward directions in which he may be flying in any line when the galvanometer reads zero. If the dial be turned slightly, say to the right, the pointer of the galvanometer will move from zero; and the galvanometer connections can be made so that this motion will also be to the right when the vessel is moving forward on the course indicated by the initial position of the dial switchboard. If the plane be flying backward on this course, the flux through the armature is reversed in direction; and, with the same connections, the motion of the pointer will now be opposite to the motion of the dial.

Contrary to what might be supposed, there is no difficulty in obtaining a galvanometer at once sufficiently sensitive and rugged to be used under the conditions prevailing in an airplane. The galvanometer used is of a standard commercial type, double-pivot spring construction, giving one millimeter deflection for one hundredth of a milliampere. Its resistance is about 22 ohms. Being always in circuit with a resistance less than its critical damping resistance, the motion of its pointer is always dead-beat. The vibration and jarring to which the instrument is subjected in flight are not great, being actually less than are to be found at the instrument board of an auto-

mobile, where instruments of this and similar types are frequently installed.

The earth inductor itself is shown in Fig. 10 (photograph). It is installed in the fuselage, behind the second seat. The supporting board *1* runs athwart the ship and is clamped to the upper wooden

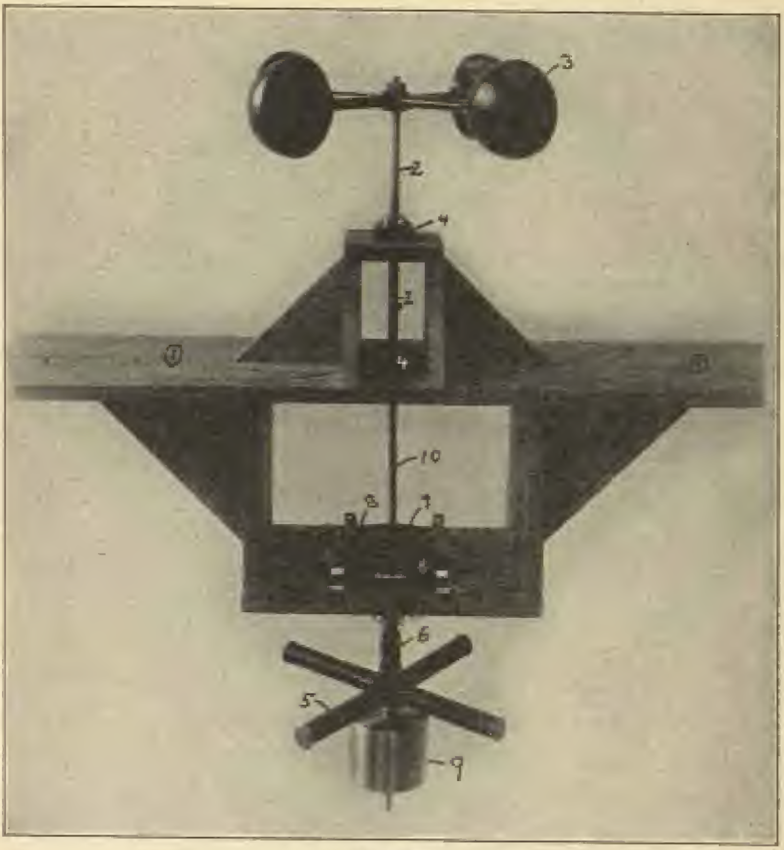


Fig. 10

members (longerons). Through the upper fabric of the fuselage (the turtle back) projects the driving axle *2* bearing the four-cup propeller *3*. This axle runs in ball bearings *4* of the self-aligning type. The axle *2* is joined to the axle *6* by a short piece of flexible shaft *10*.

A decided novelty is introduced in this invention in the use of an iron core in the armature. For an iron-cored armature revolving in

the earth's field the conditions differ in one important respect from those obtaining in the dynamo; in the earth's field we must reckon with free poles in the armature, and a consequent self-demagnetizing effect. This effect is a minimum if the iron is in the form of a rod long as compared to its diameter. This leads to a cross-shaped armature *5*, as shown in the photograph. The arms are of 3½ per cent. silicon steel, which has, in a field equal to the earth's horizontal component, a permeability of about four times that of ordinary soft iron. The residual magnetism is also considerably less. The four arms of the cross are carried by a central hub of the same material. The arms are 1 cm. in diameter, and measure 20 cm. from end to end of a pair of opposite arms. For a rod of these proportions the demagnetizing coefficient is small enough to allow a permeability of about ten times that of a cube of the same material.

Upon each of the four arms there are 500 turns of No. 20 B. & S. copper wire, silk enamel insulation. The winding is of the closed coil type, with a four-segment commutator. The total resistance of the wire on all four arms is 3.2 ohms, and the resistance through a pair of opposite commutator segments 0.8 ohm. At 20 revolutions per second, the electromotive force is 8 millivolts.

The armature and commutator are carried by the axle *6*, which runs in a thrust ball bearing mounted in the gimbal rings *7*. There are four collecting brushes of carbon, spaced 90° apart, on a mounting which swings with the axle and commutator. By turning the whole gimbal system by means of the slots and screws *8* the brush system can be set at any desired angle with respect to the vessel in which it is installed. The resistance of the armature through a pair of brushes is from 1 to 1.5 ohms.

Below the armature *5* is a brass weight *9*, weighing about a kilogram. The length of the pendulum thus formed is short enough to give a time of (half) swing of about one third of a second. The bearings for the gimbal rings *7* are provided with leather friction washers, by tightening which any desired degree of damping may be applied. It is usual to damp the pendulum so that it will execute from six to eight half-swings before coming to rest after a displacement of about 20°, occupying from two to three seconds in the

process. This allows sufficient looseness to permit response to a small angle of tilt, and also sufficient damping to insure that the oscillations do not continue beyond the time required for the plane to regain its level.

On rounding a curve centrifugal force will, of course, deflect the pendulum somewhat from the vertical; but such centrifugal force is removed as gradually as it is applied, and by the time the plane comes again into a straight course the axis is stationary in a vertical position.

The natural time of swing of the armature pendulum as arranged is, as has been said, about one third of a second. The time of roll or pitch of even the smallest planes is several seconds, and for the larger planes still longer. The disturbances arising from the driving mechanism have a period of about one twentieth of a second. The pendulum is sufficiently massive to resist forced vibrations of the latter period; and the oscillations of the plane itself are too remote in period to produce any sensible effect. A certain amount of gyrostatic action at the usual speed of revolution (1,200 r. p. m.) contributes materially to the stabilizing action. Excess of gyrostatic action is undesirable, as its effect is to lengthen the period of swing and bring it too near that of the oscillations of the plane.

Actual experiment is necessary to appreciate the very satisfactory degree of stability possible of attainment by an apparatus of this nature.

It might be supposed that there would be a small quadrantal error in an electrical system such as described, due to the fact that the armature is sending out a current which is not constant for different azimuths of the vessel, and consequently the reaction of the armature on the earth's field would be variable. When one set of brushes alone is functioning the electromotive force E is applied to opposite corners of the square frame (Fig. 7). If R be the resistance of one arm of the frame, the equivalent resistance of the whole frame is also R, and the current output of the armature is $E/(R+r)$, where r is the internal resistance of the armature. If, as in Fig. 3, both pairs of brushes are equally active, the voltage of each pair is $0.7E$. The resistance in the square frame encountered by each voltage is that of

one quadrant, or R; hence the total current coming from the armature is $1.4E/(R+r)$.

Laboratory tests fail to show such an error, which, in fact, is non-existent. Though the current output of the armature varies, the distribution of the current in the armature varies also, so that its

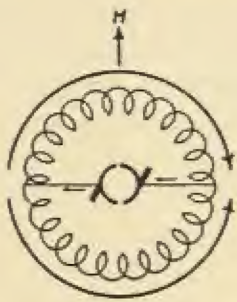


Fig. 11

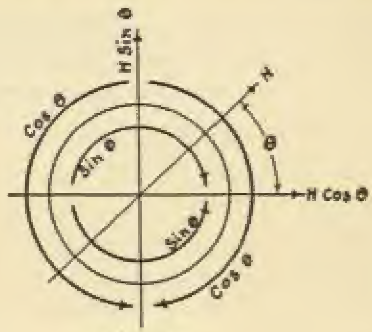


Fig. 12

$\sin\theta - \cos\theta$	$\sin\theta + \cos\theta$
$\sin\theta + \cos\theta$	$\sin\theta - \cos\theta$

Fig. 13

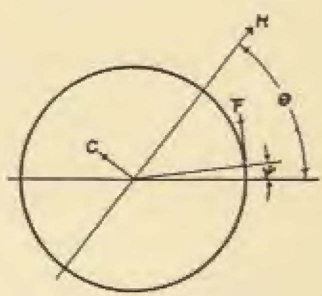


Fig. 14

integrated cross-field reaction is constant. This is readily shown by considering the case of a Gramme ring, of which the compass armature is a special and degenerate case.

Fig. 11 represents such a ring, H being the direction of the earth's field. Current will flow as shown by the arrows, producing a cross-field horizontally directed.

If the field be inclined at an angle θ to the horizontal, as in Fig. 12, we may resolve it into two components, $H \sin\theta$ vertically and $H \cos\theta$ horizontally. If the intensity of the currents in each half of the ring in Fig. 11 be taken as unity, the intensities of the similar currents generated in Fig. 12 will be $\sin\theta$ and $\cos\theta$, respectively. These currents are taken off by two pairs of brushes, and encounter the same external resistance in the dial switchboard. We may consider these component currents as superposed, the intensities in the four quadrants being shown in Fig. 13.

Let the radius of the ring be unity and let L be the inductance per unit arc of the ring. Then, representing the current intensity at any angle ψ (Fig. 14) by i, the total cross-field C will be proportional to

$$2L\int_0^\pi i\cos(\theta-\psi)d\psi$$

where $\theta-\psi$ is the angle between the reaction-field of the element of the ring at ψ and the perpendicular C to the field H.

Using the values of i in Fig. 13 this integral breaks up into two.

$$2L(\sin\theta+\cos\theta)\int_0^{\frac{\pi}{2}}\cos(\theta-\psi)d\psi$$

$$+\,2L(\sin\theta-\cos\theta)\int_{\frac{\pi}{2}}^{\pi}\cos(\theta-\psi)d\psi$$

$$=2L\Big[(\sin\theta+\cos\theta)^2+(\sin\theta-\cos\theta)^2\Big]$$

$$=4L.$$

Hence, the resultant cross-field reaction of the ring is independent of the course-angle θ, and there is no quadrantal or other segmental error from this source.

The instrument as above described will give about one millimeter deflection at the galvanometer for five degrees change of course, the sensitivity being a little greater at the cardinal points than in the middle of a quadrant. This sensitivity is capable of a reasonable amount of increase, if desired, by using a larger armature with a greater number of turns, or by increasing the speed of rotation, but for the present state of the art of aërial navigation the instrument is sufficiently sensitive.

The total weight of the apparatus as shown in Fig. 10 is about five and a half (5½) kilograms.

Excessive tilt is prevented by a guard ring (not shown in Fig. 10) supported by guy wires in such a position as to intercept the motion of the lower projecting end of the axle 6 when the angle of tilt exceeds 20°, an angle not usually reached in ordinary flying. The great demand for a compass comes from cross-country and cloud-flying—not from acrobatic maneuvers.

Air tests of this instrument have been made during its development at one of the flying fields in this country, the latest series being completed during the week of October 24, 1921. This last series of tests has demonstrated that the instrument as here described is reliable in the air; that the needle of the galvanometer can be read during pitching to a half millimeter, corresponding to two and one half (2½) degrees change of course. It appears to be impossible for a pilot to roll his plane without also slightly veering it sufficiently to cause an oscillation of the needle of about one (1) millimeter on either side. The needle is unaffected by a vertical drop or "bump." On rounding a curve centrifugal force deflects the needle; but such force diminishes, on leveling up, at a rate sufficiently slow to allow the damped pendulum to become stationary by the time it reaches the vertical. A sharp veer of the plane will cause a slight oscillation of the needle, which is damped out in about three (3) seconds. Due to the constant slight vibration of the plane, the damping of the pendulum produces no loss of sensitivity, the pendulum being shaken into the vertical from even a small displacement.

In the opinion of those pilots who have tested the instrument some form of electrical drive would be preferable to wind power, as affording a higher speed and greater sensitivity, and being more uniform. Many planes carry electrical generators. No disturbance of the compass would arise from the rotating field of an A.C. motor mounted on the same axle, as the flux due to the stray field would rotate at the same speed as the earth inductor, and be therefore invariable with respect to the coil.

Certain mechanical improvements in detail have been shown by the latest air tests to be advisable. An invention, like a work of art,

is never finished. Metallic brushes will probably be preferable to carbon for a long flight; and a commutator of hardened steel will probably save frequent replacements. The question of brush wear is naturally more important at eight (8) millivolts than at ordinary commercial pressures.

Addenda.

Since the date of the presentation of the foregoing memoir certain additional and correctional material has accumulated.

A comparative examination of various forms of commutator and brushes has shown that a mica-filled brass commutator and brushes of carbon are by far the most satisfactory combination. Such an arrangement has been run in the laboratory at 20 revolutions per second for 146 consecutive hours with a variation of less than one degree in the compass reading at any time during the test. The wear on the brushes was trifling. One hundred hours' flying is about the maximum service obtained from an airplane before complete overhauling of its engine is necessary. These additions have been incorporated in the text.

U. S. Bureau of Standards,
Washington, D. C.,
November, 1921.

"This instrument is a part of the programme set by the Air Service, in its attempt to put the navigation of the air on a basis as trustworthy as that of the water. The study of the problem was undertaken by the Bureau of Standards at the request of the Air Service, and the expense incident to the development of the final successful model was defrayed by Air Service funds. The flying tests were carried out with the coöperation of the Engineering Division of the Air Service at McCook Field, Dayton, Ohio."

THE PROBABLE ACTION OF LIPOIDS IN GROWTH.

By D. T. MacDOUGAL.

(*Read April 21, 1922.*)

Accumulating evidence, including biochemical tests of the occurrence of lipoids in cells, especially abundant in the accretion stage of growth, microscopic and ultra-microscopic examinations of plasma and wall, the use of reagents which would displace or liquefy lipoids in the modification of permeability, all support the conclusion that the lipoids constitute the external layer of the plasma sending a penetrating meshwork into both the plasma and the wall.

The implied view would make the lipoids the fundamental structure of protoplasm and the primary factor in all exchanges between the cell and the medium. The facts cited cause renewed interest in the original proposal of Quincke in 1888 and of Overton in 1895 and 1899 as to the lipoid theory of the plasmatic membrane.

Additional information concerning the nature and action of some of the common lipoids, and of the nature of hydration, together with exact determinations of the influence of various ions on permeability, gives opportunity for the consideration of the subject from new angles.

1. The experimental results which concern the matter and which are described in the present paper have been obtained by two methods. Measurements of endosmose in artificial cells, with a plasmatic lining including lipoids, have been made. These results have been correlated with measurements of the hydration reactions of biocolloids, and living and dried cell-masses in the solutions used in the osmotic tests.

2. Lecithin incorporated in the plasmatic jelly layer of an artificial cell has but little effect on the osmotic action. The same substance deposited as a layer between the plasmatic jelly and the outer wall lessens the permeability of the system and increases the osmotic effect.

3. The use of saponin or solutions which liquefy or displace lecithin in the cell contents or external layer increases permeability, presumably by increasing hydration, and lessens osmotic action. This is in agreement with the results of Boas and Kahho.

4. The nature of the action of saponin on artificial cells suggests that this substance affects the permeability of the outer clay walls of artificial cells. The hydration reactions of living and dead cell-masses measured with the auxograph show a similar influence on the wall of the plant cell.

5. Dried plates of biocolloids which show many similarities in hydration to the action of protoplasm, but which contain no lipoid, are rendered less permeable when swelled in a saponin solution. Such a solution increases the permeability of plant cells. The results support the inference that this effect is due to the action of the solution upon the lipoid constituent.[1]

6. The artificial cell as used in the experiments shows accelerated action when a lipoidal layer is deposited between the plasmatic jelly and the outer clay wall. With cell-contents of NaCl 0.01*M* immersed in $CaCl_2$ 0.001*M*, negative osmose, then positive osmose, occurs. The tonicity of the cell may be seen to increase from 0.003 to .005*M* KCl. The "life" or period of activity of such a cell may extend over a period of 60 to 80 days with renewal of the immersion liquid, but not of the cell-contents. Anomalous osmose may be exhibited by the outer wall, which is semi-permeable to sugar, asparagin, and other organic substances, but the action of the cell when the plasmatic layer is added is positive.

7. Living cell-masses which show a water deficit or a hydration capacity of 40 per cent. and which remain turgid when swollen take up only about one fourth of this amount of water when increased permeability sets up shrinkage in saponin 0.005*M*. The effect of the saponin decreases with the concentration to a minimum at about 0.0002*M*.

8. The effect of the saponin upon permeability as measured by the swelling which ensues before shrinkage begins is not definitely accel-

[1] Quincke, G., *Ann. d. Physik u. Chemie*, N. F., 35, 580, 1888. Overton, E., "Osmotische Eigenschaften," *Vierteljahrsschr. Naturforsch. Ges.*, Zurich, 40, 1, 1895. See also "Ueber die allegemeinen osmotischen Eigenschaften d. Zelle" by this author in the same publication, 44, 110, 1899.

erated by KCl in living cell-masses of *Opuntia*. This result is not in agreement with Boas, who found that the salt accelerated the action of saponin upon cells of *Tradescantia*.

9. The hydration of such living cell-masses is not definitely affected by variations in acidity up to PH 2. No positive increases were noted in KHO in weaker concentrations, but swelling was lessened in 0.01N.

10. Dried cell-masses display a minimum effect from saponin at about the same concentration as the living material, but the swelling which is maintained increases with the concentration to about 0.005M.

11. The hydration of dead cell-masses was not definitely affected by HCl at 0.01N, but at weaker concentrations, 0.001 to 0.0002N, the swelling was greater than in water. After the neutral point is passed some increase is to be noted in KHO 0.001N, which reaches its maximum at 0.01N.

12. The hydration reactions of dead cell-masses of *Opuntia* in acidified solutions are those which might be displayed by a biocolloid in which the protein and pentosan components were nearly equal in quantity. The hydration reactions of living cell-masses are such that permeability is increased and swelling lessened in hydroxide at 0.01N and 0.005N. This reaction, like that of saponin, is reversed in the dead cell-masses in which swelling increases with the concentration.

Cytologists have for the most part considered the so-called plasmatic membrane as a peripheral layer not separable from the cytoplasm and by implication to consist of proteins. Seifriz regards this suppositious membrane as a highly viscous layer about 1 μ in thickness.[2] In the recent notable contribution by Hansteen-Cranner the peripheral layer of plasmatic substance is regarded as lipoidal, consisting of a disperse phase of hydratable material not soluble in water, in a continuous phase of water-soluble lipoid. This formation is continuous with a fundamental lipoid meshwork of the plasma. A lipoid meshwork from the peripheral layer is also supposed to extend through or into the cell-walls. The conclusions thus briefly noted are held to sustain the general contention of Overton as to the lipoidal

[2] Seifriz, W., "Observations on Some Physical Properties of Protoplasm by Aid of Microdissection," *Annals of Bot.*, 35: 269–296, 1921.

character of the plasmatic membrane.[3] Such an arrangement of lipoids would be one which would permit the passage of both water-soluble and fat-soluble substances into the cell.

Czapek's notable contributions to this subject show that lipoids are especially abundant in meristem and in nearly all cells in the accretion stage of growth. Czapek places himself in the position of considering the plasma as essentially a lipoidal structure.[4] Walter, in confirmation of Biedermann, found that the plasmatic mass of plants is not readily digested by proteoclastic enzymes until the lipoid, which is held to be in a fine state of dispersion, is first extracted by a fat solvent. He holds that his evidence is against the conclusion that the lipoids are localized in a peripheral layer in the cell.[5]

The presence of lipoids in the cell colloids in the accretion stage of growth would, it seems, almost inevitably result in their accumulation in the peripheral portion of the plasmatic mass in accordance with the laws of surface tension. That such a layer does exist was concluded by Boas, who published a preliminary paper to this effect in 1920 and his detailed observations upon which this conclusion was reached in 1921. The experiments were based upon the known reactions of lecithin and cholesterin to neutral salts and to saponin. Measurement of fermentation in yeasts and of the decolorization of cells of higher plants are made the basis for the assertion that a proteinaceous membrane in the cell is highly improbable, and that lipoids are concerned in the exchanges of the cell with the medium.[6] Although this writer found that non-conductors in hypotonic solution retarded the action of saponin on the supposed lipoidal layer, and that hypertonic solutions of cane sugar, for example, accelerated it, the

[3] Hansteen-Cranner, B., "Beiträge zur Chemie und Physiologie der Zellwand und der plasmatischen Grenzschichten," *Ber. d. Deut. Bot. Gesell.*, 37, Hft. 8, 380–391, 1919.

[4] Czapek, F., "Zum Nachweise von Lipoiden in Pflanzenzellen," *Ber. d. Deut. Bot. Gesell.*, 37: 207–216, 1919.

[5] Walter, H., "Ein Beitrag zur Frage der chemischen Konstitution der Protoplasma," *Biochem. Zeit.*, 122: 86–99, 1921.

[6] Boas, Fr., "Beitrage zur Kenntniss der Wirkung des Saponins auf die Pflanzliche Zelle," *Ber. d. Deut. Bot. Gesell.*, 38: 350–353, 1920. Also, "Untersuchungen ueber die Mitwirkung der Lipoide beim Stoff-Austausch der Pflanzlichen Zell," *Biochem. Zeit.*, 117: 166–214, 1921.

possibility of the action on other colloids than proteins and lipoids is not considered. Kahho published the results of his tests as to the permeability of roots of yellow lupine to neutral salts late in 1921. He confirms the series as to permeability of kations which runs K>Na>Li>Mg>Ba>Ca, in which the greatest penetrability is shown by potassium and the least by calcium. It is also seen that the interferences are such that each kation is retarded by those to its right, and to a degree proportionate to its distance to the right, and that the greatest retardation is by the kations which show the greatest coagulating action on colloids. The kations which have the least coagulating action on colloids penetrate most rapidly. The anions retard the colloidal or coagulative action of the kations in a series, citrate<sulphate<tartrate<Cl<NO_3<Br<I, in which the effect is least with the citrate and most with the iodine. That is, each kation has the greatest effect when combined with the citrate and least with the iodine. As a further consequence, the citrates have the least penetrability and the salts of iodine greatest. It is held that the behavior of the roots in a weak alkaline solution supports the conclusion of Hansteen-Cranner as to a lipoidal layer.[7] The greater expansion and contraction of growing roots in weak alkaline solutions is attributed to the solution or displacement action of such solutions on the lipoidal meshwork of the walls, rendering them more contractile.

The results cited above are not decisive or final when taken separately. Their concurrence lends substantial support to the contention that the lipoids are a prime factor in the exchanges between the cell and the medium. The establishment of the fact that such a lipoid as lecithin may be present as an emulsion consisting of a disperse medium soluble in water and of a disperse phase swelling in water, the entire system displaceable by fat-solvents, would furnish a plasmatic membrane or peripheral layer through which both salts and fatty substances might diffuse. Lecithin, for example, is supposed to absorb about 40 per cent. of its volume in hydration in water, is soluble in chloroform, alcohol, benzene, carbon disulphide, etc., and has the power of combining with both acids and bases.

[7] Kahho, H., "Ein Beitrag zur Permeabilitat des Pflanzenprotoplasmas für Neutralsalze," *Biochem. Zeit.*, 120: 284–303, 1921. See also p. 125, same volume.

The importance of the entire matter is such that two series of experiments were designed in my own laboratory, the results of which might have a bearing upon the above conclusions. In one the auxograph was used to register variations in thickness indicative of changes in turgidity of cell-masses subjected to neutral salts and other solutions. Next lipoids were introduced into the construction of the artificial cell recently designed, and the effect of such substances upon permeability of plasmatic layers of cell colloids under the influence of salts, saponin, and soaps was determined.

Brief mention has already been made of the artificial cell used.[8] The cell in question was of a design in which clay, porcelain, alundum, or wooden thimbles representing various degrees of porosity were used to represent the external wall, while the plasmatic layer could be represented by a plasmatic lining layer of any jelly or mixture of jellies. The thimbles were fitted with an osmometer head consisting of a stopper pierced with two holes, in one of which was fitted a filling funnel with stopcock, and the other with an outlet tube bent to the horizontal immediately above the stopper. Such an arrangement permitted the measurement of endosmosis by the amount of liquid forced out and caught in a small graduated receiver. The greater number of experiments were made with the clay thimbles used in the Livingston evaporimeter (Fig. 1). Cells of this type lined with agar treated with tanning reagents and fitted with vertical outlets to show pressure, designed by Professor H. M. Richards, have been in use for some time in the Botanical Laboratories of Barnard College. As arranged in the work described here, the pressures in the cell were never more than that of 12 or 15 mm. of water.

The arrangement of this artificial cell was begun by washing the thimbles in warm distilled water and preparing a liquid mixture of the materials for the plasmatic lining layer. About 10–12 cc. of this material was poured into the warm, moist thimble at a temperature of 40–50° C., the osmometer head put in place, and the thimble turned in the hand in a horizontal position. If a coating of 2 or 3 mm. in

[8] MacDougal, D. T., "The Distentive Agencies in the Growth of the Cell," *Proc. Soc. Exper. Biol. and Med.*, 19: 103–110, 1921. See original description of this cell in the *Report of the Dept. of Bot. Res., Carnegie Inst. Wash.* for 1921.

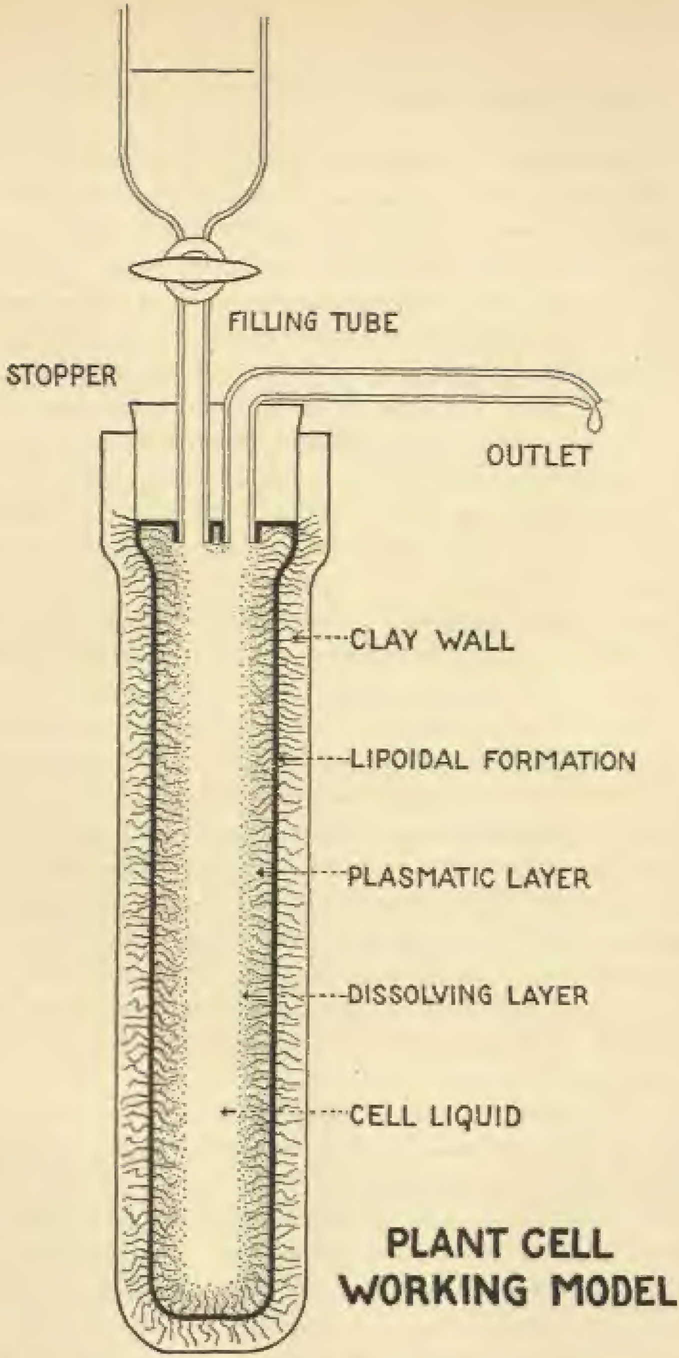


FIG. 1. Artificial cell designed to illustrate variations in outer wall and plasmatic layer. The clay wall is that of a filter thimble such as is used in the Livingston evaporimeter. The lipoidal layer is drawn heavily out of proportion to illustrate more clearly the processes extending into the wall and the plasmatic layer. The plasmatic layer in the experiments described in this paper was composed of agar, agar-gelatine, or modified mixtures. The cell was submerged to the top of the stopper in operation.

thickness was desired, this was continued until all of the jelly had set. If a thinner layer was required, the stopper was removed after two or three minutes and most of the liquid poured out. Uniformity was attempted in the cells in any series.

After cooling for ten minutes, the cell contents were poured in and the preparation set in the immersion solution so that the outlet tube was horizontal and no siphoning action was possible. Readings were generally made at 24-hour intervals unless otherwise specified.

The clay thimble is so readily permeable to salts of potassium and sodium at 0.01*M* that no action of this wall alone followed a filling with such solutions except a direct outward diffusion. It is much less permeable to organic compounds, such as sugar, asparagin, saponin, soap, etc. A comparison of its action with the results given by Bartell would seem to indicate that the clay wall has pores less than 1 μ in diameter.[9] The positive action of the thimble with sugar may be illustrated by the fact that when filled with a 2 per cent. solution and immersed in water at 18° C. a column 3 mm. in diameter was raised to a height of 35 mm. in 30 minutes. Asparagin, saponin, and urea at 0.01*M* also gave positive pressures.[10]

Solutions of agar 0.3 per cent., of gelatine and of potassium oleate as cell contents resulted in endosmose in clay thimbles without any lining layer. Further tests showing the negative osmose which might take place through the walls of the clay thimbles were made with calcium solutions. One lot of thimbles were filled with $CaCl_2$ at 0.01*M* and set in water, while an equal number were filled with water and set in the calcium solution. The calcium solution inside the cell set up negative osmose which retracted the horizontal column of water in the outlet tube, and the cell containing water showed exudation caused by the passage of water through the walls from the calcium solution in which it was immersed.

Tests with cylinders of turned wood with walls 3 mm. in thickness showed positive osmose when filled with the calcium solution, as did also filter paper thimbles (double thickness Whatman). It is evident, therefore, that this negative osmose is to be attributed to the

[9] Bartell, F. E., "Pore Diameters of Osmotic Membranes," *Jour. Phys. Chem.*, 16: 318, No. 4, April, 1912.

[10] Stern, K., "Ueber negativen osmosen und verwandten Erscheinungen," *Ber. d. Deut. Bot. Ges.*, 37: 334–343, 1919.

size of the pore or the composition of the wall. The repetition of the tests with calcium chloride at 0.001*M* as an immersion fluid on a clay cell filled with water gave an exudation of 1.3 to 1.6 daily for twenty days. At the end of this time the external liquid was replaced by one more concentrated at 0.01*M*. The rate rose slightly during the first day, then fell away to 0.2 c.c. on the fifth day.

Another interesting case was that in which a clay cell containing sodium at 0.01*M* was set in a calcium solution 0.001*M*. The initial action was a negative osmose resulting in the loss of water from the stronger sodium to the weaker calcium solution outside the cell during the first 24 hours. The action was now reversed and endosmosis began at the rate of 0.2 c.c. daily, which rose to 1.4 c.c. daily on the fourteenth day, after which the rate fell off. Replacement of the immersion solution with a more concentrated calcium solution at 0.01*M* did not alter this process, as the rate continued to decrease to 0.2 c.c. on the tenth day.

After such measurements had been made of the porosity of the clay walls, lining layers of various mixtures of biocolloids were placed in them and their absorption capacity measured by the amount of water delivered by the outlet tube.

The examples given below are sets selected at random from a large number in which no radical departure from the average behavior was found. Unless otherwise stated the cells were immersed in water, and the cell contents were at 0.01*M*. Endosmose measured as excretion is given below in c.c.

	KCl.	NaCl.	HCl.	Asparagin.	Water.
Gelatine	2.4	2.6	1.6	2.9	1.5
Agar	4.5	3.6	5.3	3.1	1.6

A special set of cells for testing the comparative effects of sodium and potassium chlorides at 0.01*M* with a plasma of agar-gelatine-potassium oleate was operated with results as follows:

Contents.	Outside Liquid.	Amounts Exuded.	Average.
Water	Water	2.2, 2.5, 2.8 c.c.	2.5 c.c.
KCl 0.01*M*	"	3.2, 6.9	4.9 "
NaCl 0.01*M*	"	2.7, 2.3	2.5 "

Repetition of these experiments gave no data varying notably from the above. All were carried out at 15° C.

A fourth plasmatic layer was prepared consisting of agar 5 g., gelatine 2 g., potassium oleate 5 mg., and lecithin 25 mg., and parallel series in potassium and sodium chloride were run at laboratory temperatures of 14 to 18° C. with results as follows, in which the total excretions for three cups with each salt are given:

KCl 0.01*M*	5.8 c.c.	8.4 c.c.	9.4 c.c.	Total in 140 hours 23.2 c.c.
NaCl 0.01*M* ...	5.2 c.c.	6.7 c.c.	8.2 c.c.	" " " " 20.1 c.c.

It is to be seen that the action of the sodium and potassium salts is most nearly equivalent in gelatine, that the greatest differences are shown in a layer of agar, and agar-gelatine soap where the amount of excretion from a potassium cell may be double that of one filled with sodium. However, when lecithin is incorporated in the mixture, the difference between the action of the two bases is not very great.

The several processes which are integrated in these results may be simply stated as follows: First, the plasmatic layers begin to go into solution as soon as the cell is filled and the resulting osmotic action will be one characteristic of such solution. About 10 per cent. of an agar-gelatine soap layer 2 or 3 mm. in thickness would be dissolved at the end of the fourth day, but after this saturation point of the cell contents is reached the plasmatic layer dissolves slowly as new supplies of water are absorbed so that cells have been seen to operate for 60 to 80 days.

It has been pointed out previously that such a cell with a plasmatic layer of agar-gelatine-potassium oleate mixture has at the beginning a tonicity by which it shows endosmosis in a 0.03*M* solution of potassium chloride. After some action in this concentration it may be moved by stages to a solution of 0.005*M* from which it will absorb water.[11]

[11] MacDougal, D. T., "Distentive Agencies in Growth," *Proc. Soc. for Exper. Biol. and Med.*, 18: 103–110, 1921.

[12] See Fitting, H., "Untersuchungen ueber die Aufnahme von Salzen in die lebende Zelle," *Jahrb. f. Wiss. Bot.*, 56: 1–64, 1915.

Stiles, M. A., and Kidd, F., "The Comparative Rate of Absorption of Various Salts by Plant Tissue," *Proc. Roy. Soc. Lond.*, B, 90, 487–504, 1919.

Kahho, H., "Ein Beitrag znr Permeabilität des Pflanzenplasmas für die Neutralsalze," *Biochem. Zeit.*, 120, 284–303, 1921.

The above applies to cells filled with water and immersed in water. When the contents include substances already in solution, such as the salts of potassium and sodium, the action of these salts on the colloids both as to their solution and hydration are to be taken into account. As hydroxide at 0.01*M* potassium is found to retard the hydration of agar more than sodium. Whether this would be true of the designated colloidal mixture, and as a chloride, has not been tested, but probably this action would be reversed.[12]

The ionic velocity and speed of penetration of the potassium would be greater, and its superior osmotic action would result in a greater endosmosis than in the cell containing sodium.

That solution of colloids, the penetration of the pores by colloidal matter, and the action of salts and other compounds act progressively may well be inferred from the long-continued swelling action of these biocolloids in dilute salt solutions and amino-compounds, on the one hand, and on the other by the fact that when biocolloidal cells are set in action the rate of endosmosis rises until some time in the third 24-hour period, no change having been made in the preparation meanwhile. A decline in the rate takes place during the fourth day, but the decrease is so slow that endosmosis may continue for 60 to 80 days. Such action is illustrated by the following figures:

Contents.	Amount of Endosmosis.					
	1st Day.	2d Day.	3d Day.	4th Day.	Re-filled 5th Day.	6th Day.
Water	0.3	1.5	2.8	1.7		
Urea 0.01*M*	0.7	1.8	3.5	2.2	2.6	2.6 c.c.
Asparagin 0.01*M*	0.8	2.4	4.5	3.0	1.6	2.2

The above procedure, it should be noted, is one that characterizes the agar-gelatine-soap biocolloid and is not shown by agar in which endosmosis falls off rapidly.

The foregoing tests serve to illustrate the operation of the artificial cell which it was proposed to use in determining the possible action of a peripheral layer of lecithin or lipoids in the absorption of salts by the plasmatic layer. Lecithin had been incorporated with

the other main components of a lining layer in one series of preparations, but the effects of its action as minute globules distributed throughout the mass were indeterminate, as noted above. A special series prepared in such manner as to secure a lipoidal layer external to the plasmatic mass and lying next to and in the wall was next prepared.

Clay filter thimbles which had been cleaned and warmed in distilled water were drained a few seconds, then 4 c.c. of a 2 per cent. solution or emulsion of lecithin was poured into it. After stoppering the thimble was turned in the hand for about two minutes in such manner that the lecithin was made to bathe the entire inner surface repeatedly. About 3 c.c. of liquid would be poured out when the stopper was removed, but this would be of a lighter hue indicative of the fact that some of the lipoid had been taken up by the cup, where it would be held in the inner surface layer. If the properties of the lecithin layer alone were to be tested, the osmometer head would be put in, the desired solutions poured in through the funnel tube, and the thimble set in the immersion fluid. If a more complete simulation of the conditions in the cell were desired, the thimble treated with lecithin received about 10 c.c. of liquid jelly, and after being again stoppered was turned for another two minutes, when this was also poured out, leaving a thin layer on the wall, and the osmometer head put in place. Such cells now included a great central vacuole to receive any desired cell-contents, had a plasma of an agar-gelatine-soap mixture, and a peripheral layer of lipoid which had undoubtedly penetrated the wall to some extent. The pores of the clay walls were so large that solutions of potassium and sodium would pass through them readily setting up no pressures.

That the deposit of lecithin had effectually closed the larger pores was evidenced by the fact that when cells treated with lecithin only were filled with potassium chloride or sodium chloride at $0.01M$, the average endosmosis in 24 hours as measured by the excretion for the potassium was 6 c.c. and slightly less than 4 c.c. for the sodium. These figures represent the integration of the action of the two kations on the lipoid and their osmotic pull.

Next a series of cells with a lecithin layer and an inner layer of agar and gelatine were arranged to test the influence of saponin on

the action of potassium solutions. Two cells were used for each case and the endosmosis for 20 hours was as below:

Cell-contents.	Immersion.	Excretion or Endosmosis.
Water	Water	1.1, 1.2 c.c.
KCl 0.005*M*	Water	4 3 c.c.
KCl 0.005*M* Saponin .005*M*	Saponin 0.005*M*	0.2, 0.3 c.c.
KCl 0.005*M* (cell treated with lecithin only)	Saponin 0.005*M*	0.0, 0.0 c.c.
KCl 0.005*M* (lecithin only)	Water	3.3, 3.5 c.c.

One of the most noticeable features of the tests with the potassium solution as above, as well as with the sodium and calcium the results of which are given below, is to the acceleration of the action of the cells so that the amount of endosmosis was greater during the first 24 hours than with cells not treated with lecithin. This is especially noticeable in cells coated internally with lecithin only.

The measurements of the action of cells filled with sodium solution are given below:

Cell-contents.	Immersion.	Endosmose.
Water	Water	1.4, 1.3 c.c.
NaCl 0.005*M*	Water	1.6, 1.5 c.c.
NaCl 0.00f*M* Saponin 0.005*M*	Saponin 0.005*M*	0.0, 0.0 c.c.
NaCl 0.005*M* (lecithin only)	Saponin 0.005*M*	0.0, 0.0 c.c.
NaCl 0.005*M* (lecithin only)	Water	2.7, 5.6 c.c.

A series of cells with calcium as the principal salt-content showed action as follows:

Cell-contents.	Immersion.	Endosmose.
Water	Water	2.4, 0.4 c.c.
$CaCl_2$ .005*M*	Water	0.9, 0.6 c.c.
$CaCl_2$ .005*M* Saponin .005*M*	Saponin 0.005*M*	2.6, 0.2 c.c.
$CaCl_2$ .005*M* (lecithin only)	Saponin .005*M* Exosmosis	0 0
$CaCl_2$ .005*M* (lecithin only)	Water	4.0, 0.5 c.c.

The cells containing potassium were run for 20 hours, those with sodium 16 hours, and those with calcium 24 hours. The endosmosis for two cells in terms of excretion of water is given in each case.

The control pair of cells are filled with water and set in water giving the endosmostic action of the plasmatic colloids which dissolve into the central cavity. The osmotic action of the salts in cells set in water is illustrative of relative action of the three bases in these cells. The cells containing a salt and saponin in an immersion liquid of saponin were under conditions which would liquefy the lecithin layer and make it as well as the other jellies more permeable to salts of potassium and sodium. The liquefaction of the lecithin in calcium cells apparently was accompanied by the blocking of the larger pores in the clay wall, or by the coagulative action of the calcium on the agar-gelatine jelly and the lecithin. Cells with a lining layer of lecithin only showed a sufficiently low permeability as to give marked amounts of endosmose.

The above results are of direct interest in showing that if the living cell does have a peripheral layer of lipoids the treatment with saponin might well result in its liquefaction with a resultant radical change in its permeability to salts. The observations of Boas are to the effect that some of the organic contents of the cell may exercise an influence on the action of saponin.

The principal matter of importance in this connection, however, is the possible effect of the saponin on the cell colloids beside the lipoids which would alter their permeability relations. A series of cells were given the lipoidal treatment, then lined with gelatine-agar jelly. The following results were obtained:

Contents of cell.	Immersion.	Excretion or Endosmosis.	
Water	Water	8.5 c.c.,	8.4 c.c.
Cane sugar 5 per cent	Water	8.7	8.0
Cane sugar 5 per cent	KCl 0.01*M*	4.5	6.7
Cane sugar 5 per cent. Saponin .0025*M*	KCl .0025*M* Saponin .0025*M*	1.5	1.8

The clay walls of the cells are only slowly permeable to sugar and the clay thimble with no lining layer would probably show endosmose equivalent to the amount excreted when immersed in water. When the immersion fluid contains potassium chloride the endosmose is the resultant of the opposing action of the salt and sugar. If now the sole action of the saponin were to liquefy the lecithin and render

it more permeable to the potassium, the amount of endosmose by the action of the sugar in the cell contents would have been increased. It was lessened, in fact, suggesting that the saponin exercised some influence on the condition of the wall or the plasmatic colloids. A special series to test this matter was now arranged, as noted below:

Cell-contents.	Immersion Liquid.	Excretion.		Comment.
Water	Water	1.4 c.c.,	1.9 c.c.	Averages for 3 days
KCl 0.005M	Water	1.9	2.3	Averages for 3 days
KCl 0.005M	Saponin 0.005M	0.7	1.8	One cell on 2 days. Two other cells gave negative results
KCl 0.005, Saponin 0.005M	Saponin 0.005M	0.0	0.0	No positive results
Sugar 5 per cent.	Water	2.2	2.2	Averages for 2 days
Sugar 5 per cent., Saponin 0.005M	Saponin 0.005M	0.0	0.0	First day
		0.4	0.4	Second day
		0.3	0.5	Third day
		0.5	1.2	Fourth day
KCl 0.005M, K-oleate 0.005M	K-oleate 0.005M	0.8	0.8	First day
		0.6	0.0	Second day

The presence of the saponin results in a diminution of endosmosis with either sugar or potassium chloride as the major constituent of the cell-contents. Such a result has only one explanation, that of increased permeability to both substances as a result of the action of the saponin on the clay wall and the plasmatic layer.

After the above readings had been made the cell which contained potassium and was immersed in water was shifted to an immersion in saponin 0.005M. Exosmosis resulted immediately, which would be negative osmose, as the osmotic action of the salt solution would be much greater than that of the dilute saponin without. The cells containing water only were now filled with KCl 0.05 and immersed in saponin 0.005M. One gave 0.5 c.c., then showed exosmosis. The other gave 2 c.c. and 1.8 c.c. on successive days.

An untreated clay thimble was filled with the combined sugar and saponin and set in saponin, with the result that an endosmosis of 1 c.c. was measured in the first day. Another untreated thimble filled with sugar in 5 per cent. solution gave 0.5 c.c. endosmose in 1 day, which was below expectancy for these preparations. The cell

with an agar-gelatine plasma filled with a 5 per cent. cane sugar solution gave averages of over 2 c.c. daily when immersed in water, but action ceased when it was transferred to a 0.005*M* saponin solution.

It is evident that the presence of saponin with sugar or salt solutions in the contents of the clay thimble or the cell, or in the immersion fluid, lessens endosmose, presumably by increasing the permeability of the membranes, or layers of jelly and the firmer wall.

Some of the agar (3 parts) -gelatine (2 parts) jelly used in making the plasmatic layer of the cells was dehydrated, coming down to a plate 0.12 mm. in thickness. Trios of section were hydrated under the auxograph with the following increases in thickness at 14–16° C.:

Water	2200 per cent.
Saponin 0.01*M*	2830 per cent.
KCl 0.01*M*	2250 per cent.
KCl Saponin 0.01*M*	1875 per cent.

The actual hydration in the saponin was less than in water, that in KCl was still less, while the combination of the salt and the saponin restrict hydration still more. The action of the saponin on a plasma of the above type would therefore be to lessen permeability alone and in the presence of the salt. This would tend to increase osmotic action, if the plasma alone were concerned. It must be concluded, therefore, that the saponin has no action in the artificial cell except that which would lessen permeability of the plasma and increase that of a lipoidal layer and of the porous outer wall.

Attention was now turned to the more difficult task of interpreting the action of living and dried cell-masses in solutions which might theoretically affect the peripheral layer. The measurements of Kahho were taken from roots fully hydrated in distilled water. When such roots were placed in KCl at 0.22*M* an initial shrinkage of 11 per cent. ensued within a few minutes, to be followed by an expansion which regained 4 per cent. of the shrunken length.

The flat joints of *Opuntia* were chosen as the material for my own tests, as the composition and general behavior of these plants

has been a subject of study at the Desert Laboratory for many years. Sections about 1 cm. square and having the thickness of the joint 10.5 mm. were placed in various solutions at 14–18° C. and their changes in thickness recorded by the auxograph as below:

Solution.	Increase.	Comment.
Water	31 per cent.	In 20 hours followed by shrinkage of 2 per cent. in following 30 hours
KCl 0.01*M*	31 per cent.	Continuous swelling for 50 hours
KCl 0.0075*M*, Saponin 0.005*M*	15 per cent.	In 12 hours followed by shrinkage of 8 per cent. in 40 hours
Saponin .01*M*	11 per cent.	In 6 hours with shrinkage of 7 per cent. in 44 hours

Shrinkage in the two cases treated with saponin would have reduced the sections to original dimensions in a few hours more.

It is to be noted that Boas used 2.5 per cent. solutions of saponin in the treatment of the higher plants, which would be equivalent to about a 0.003*M* solution.

A test was made with slices cut longitudinally from the median portion of the joint consisting chiefly of large, thin-walled parenchyma. The average thickness of the trios in the swelling dishes ranged from 3.5 to 4 mm. and the increase was calculated on the original measurements.

The following increases were obtained:

Solutions.	Increase.	Comment.
Water	40 per cent.	Very slight shrinkage after 30 hours
KCl 0.005*M*	38 per cent.	Very slight shrinkage after 30 hours
KCl 0.005*M*, Saponin .005*M*	8 per cent.	Decided shrinkage after 6 hours which carried the pen back near the base line in 40 hours
Cane sugar	1 per cent.	
Saponin 0.01*M*	8 per cent.	
Saponin .005*M*	9 per cent.	Decided shrinkage after 6 hours which carried the thickness back nearly to the original after 40 hours

At the conclusion of the test as above the ones hydrated in water and in KCl were put in a saponin solution at 0.005*M* with a shrinkage, most rapid in the sections first swelled in KCl, which reduced them nearly to their original dimensions.

Another series of slices of *Opuntia* about 2.5 mm. in thickness were first allowed to hydrate in water for 6 hours, in which time an increase of 35 to 40 per cent. was noted. The water in the dishes was now pipetted off and replaced with other solutions. Saponin 0.005*M* and a mixture of saponin 0.005 and KCl 0.005*M* resulted in a rapid shrinkage to about the original dimensions in 10 hours. KCl 0.005*M* had no effect on a trio of sections which quickly shrunk when the salt was replaced with saponin 0.005*M*.

The maximum swelling of thin sections of dried material of *Opuntia* was reached at a concentration of saponin between 0.001*M* and 0.005*M*. Samples which swelled 150 per cent. in water made an increase of 188 to 200 per cent. in such saponin solutions. Other samples which swelled 220 per cent. in water showed increases of 260 to 320 per cent. in saponin at the above concentrations. The increase was practically identical with that of water at 0.0002*M* and at 0.01*M* and stronger. The sections first dried and then hydrated as above showed no shrinkage at the end of 36 hours, even in the stronger solutions. The action seems to be one purely of imbibition of water by walls, mucilages, and lipoids. It is not affected by KCl.

The hydration of living sections in saponin shows the effect of altered permeability. A series of thin slices of living tissue were hydrated in a graded series as below, and after full expansion had been reached a neutral salt, NaCl, was added to increase permeability in some sections. The results are given below:

Solution.	Swelling.	Time.	Comment.
Water	33 per cent.	8 hrs.	Replaced by NaCl 0.01*M* and shrinkage set in steadily
Saponin 0.005*M*	13	15 min.	Followed by rapid shrinkage
Saponin 0.001*M*	27	4 hrs.	Followed by more gradual shrinkage
Saponin 0.0002*M*	30	4 hrs.	Replaced by NaCl 0.005*M* with shrinkage at rate identical with that in water
Saponin 0.000,04*M*	39	6 hrs.	Replaced by NaCl 0.005*M* with gradual shrinkage at same rate as in above and in water

The initial swelling of the sections is met and canceled most quickly in the stronger solutions, which increase the permeability of

the cell and allow the escape of its contents. Whether wall and plasma are equally changed is not shown. It is important to note that such increased permeability results in the stronger solutions without the addition of a salt as in Boas's experiments.

Increase of the hydrogen-ion concentration did not modify the action of the saponin in the single series of tests carried out as shown below in which thin slices of living tissue were hydrated.

Solution.	Swelling.	Time.
Water	20 per cent.	2 hrs.
Saponin 0.005*M* HCl 0.005*M*	9	20
Saponin 0.005*M* KCl 0.005*M*	9	10

The presence of the salt, however, appeared to speed up the process of imbibition and to hasten the shrinkage.

Sections of living tissue such as the above show a swelling slightly less than that in water when hydrated in acid at PH 2, due presumably to increased permeability. KHO at 0.01*M* also gives a swelling slightly less than water, at PH 12. Such a result would be much more complicated, as the hydroxide may affect lipoids as well as the proteins of the plasma. Sections which increase about 75 per cent. in water swell only 31 per cent. in KHO 0.2*M*, 67 per cent. at 0.05*M*, and 70 per cent. at 0.01*M*, and the maximum lies near this concentration, a lessening swelling appearing in concentrations of 0.005*M*.

Dried sections hydrated in a series of concentrations of HCl which swelled 240 per cent. in water did not reach this figure in HCl 0.01*N*. At weaker concentrations no graded series of values was obtained, but increases of 255 to 300 per cent. were measured. These reactions would be similar to hydration values of agar-gelatine biocolloids in which the pentosanic and the albuminous components were nearly equal in quantity. Similar sections which swelled 185 per cent. in water increased 200 per cent. in KHO at 0.01*N*, 250 per cent. in a 0.005*N* solution, 210 per cent. in a 0.001*N* solution, and gave the same value as water in KHO at 0.0002*N*. These hydration reactions are also in consonance with the relative swellings in water

and hydroxide of mixtures of agar and gelatine nearly evenly balanced.[13] Although the lipoids present would be liquefied in the stronger solutions, the effect of their presence is not discernible in the action of the dried sections in which the permeability of the wall and other layers has reached the maximum.

The numerous corrections and amendments to the plasmolytic method of estimating permeability and tonicity of cell-contents are suggestive of the complexity of the factors which enter into the exchange between the cell and the medium. An average of sixty per cent. of the osmotic pressure of the cell sap is due to electrolytes. These with the non-electrolytes affect or determine the degree of hydration of the constituents of the plasma and the wall, upon which permeability depends directly. Such action is with but little reference to the isotonic values of the substances concerned. A knowledge of the principal features of the hydration reactions of the plasmatic constituents, and of the cell-wall under the influence of cell-contents and medium is therefore fundamental to any comprehension of the passage of material through the membranes of the plant.

According to the recent work of Bartell and Sims, swelling or increase by hydration may be the result of the action of several forces. Whether their conclusion that a solution tending to exercise negative osmose increases hydration of the membrane, while conditions which shrink the membrane act positively is in agreement in all of its implications with those of Kahho as to penetration and hydration, is not yet clear.[14] Some of the newly disclosed possibilities of the intervention of the lipoids are to be taken into account in any consideration of the passage of material into or out of the cell with resultant changes in volume which constitute the essential features of growth.

[13] MacDougal and Spoehr, "The Components and Colloidal Behavior of Plant Protoplasm," *Proc. Amer. Phil. Soc.*, 39, 150–170, 1920. See pages 156, 157.

[14] Bartell, F. E., and Sims, L. B., "The Relation of Anomalous Osmose to the Swelling of Colloidal Material." *Jour. Am. Chem. Soc.*, 44, 289–299, 1922.

The Desert Laboratory,
Carnegie Institution,
Tucson, Arizona, April, 1922.

THE SMALL ENTELODONTS OF THE WHITE RIVER OLIGOCENE.

Investigation aided by a grant from the Marsh Fund of the National Academy of Sciences.

By WILLIAM J. SINCLAIR.

(*Read April 22, 1922.*)

Contributors to the literature on the entelodonts or so-called "giant pigs" are in agreement in regarding the small animal with closely crowded lower premolars (p_1 excepted) from the Titanotherium beds of the Cypress Hills, Saskatchewan, generally known as *Archæotherium coarctatum* Cope, not only as quite distinct specifically, but also as more primitive than the other American forms, Mr. Troxell in his recent paper even referring it to the European genus *Entelodon*.[1]

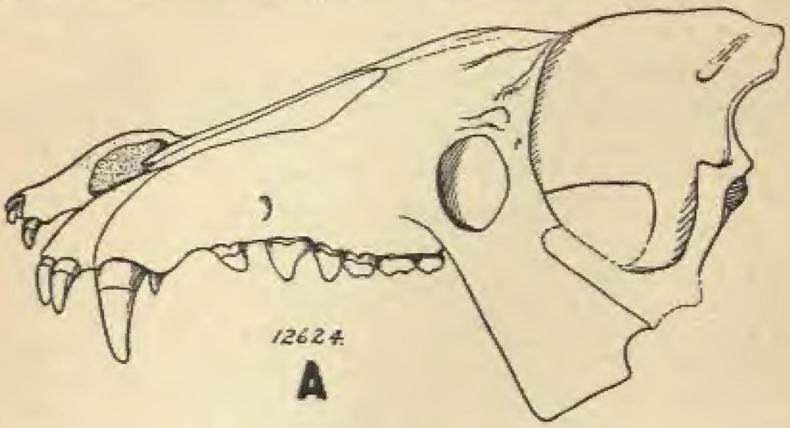
The Princeton Expedition of 1921 was so fortunate as to secure from the Oreodon beds the skull and lower jaws of a small entelodont with unspaced lower premolars (the first excepted) and reduced m^3, and a search through our own collections and those of the American Museum of Natural History, kindly placed at my disposal by Professor Osborn and Dr. Matthew, has brought to light additional material which makes it desirable to view the situation somewhat differently from that indicated above.

The recently acquired Princeton specimen (No. 12624, Figs. 1 *A*, 2, 3 *B*, 4 *A*) was found by Mr. H. R. Wanless, to whom we are indebted for the discovery of so many fine entelodont skulls during the last two summers, and is from clays intercalated in the lower zone of rusty nodules in the Lower Oreodon beds at Culbertson's[2] locality on Bear Creek (Princeton collecting locality 1016E2A) about four

[1] E. D. Cope, "Contributions to Canadian Palæontology," Vol. III., pp. 20–21, Pl. XIV., Figs. 3, 3*a*, 1891. O. A. Peterson, *Memoirs Carnegie Museum*, Vol. IV., No. 3, pp. 55–56, Fig. 11, 1909. E. L. Troxell, *American Journal of Science*, Vol. L., p. 249, 1920.

[2] See T. A. Culbertson's diary under date of May 14, 1850, in Fifth Annual Rept. Smithsonian Inst., p. 93.

and one half miles northeast of Scenic, Pennington County, South Dakota, and about a mile south of where "71 Table," the local name for a mesa-like area of high plain, ends. In this specimen all the



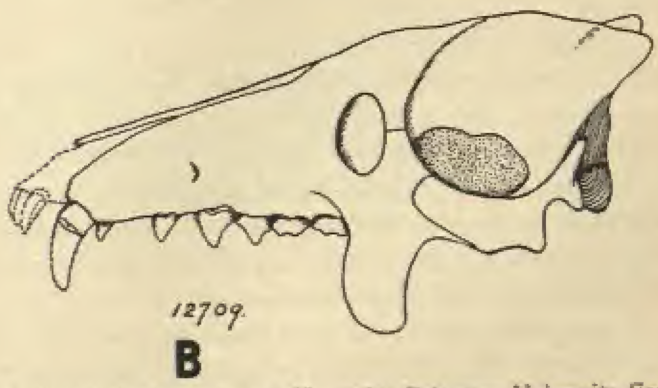
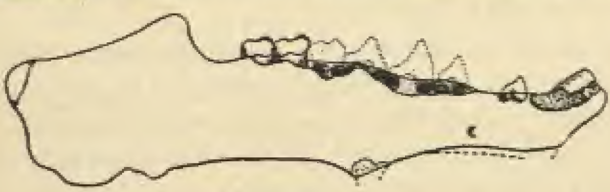


FIG. 1. *A. Archæotherium mortoni*, No. 12624 Princeton University Geological Museum. Left side of slightly distorted skull, about one fifth the natural size; view not in absolute profile. Large size-variant with reduced cuspidation in m^3 and crowded anterior lower premolars. *B. Archæotherium mortoni*, No. 12709 Princeton University Geological Museum. Left side of uncrushed skull, about one fifth natural size; view in absolute profile. Small size-variant with unreduced cuspidation in m^3.

lower teeth except the last two molars had fallen from the jaw previous to fossilization, but the alveoli of the cheek teeth are well preserved and in contact, with the exception of those for the two

roots of p_1, which are some 4 mm. removed from the canine alveolus and 14 mm. from the alveolus for the anterior root of p_2. In the upper dentition, where there is no indication of premolar crowding, a very decided simplification in the crown pattern of the third molar is found and a reduction in the size of the tooth as compared with other specimens in which a larger number of cusps are present. In the anterior row (Fig. 3 *A*, *B*, left m^3) there are two large cusps slightly joined in front by a sharp saddle, with almost no protoconule (Fig. 3 *B*) or, in another specimen, with a small development of this cusp (Fig. 3 *A*). In the posterior row, the metacone is well developed, there is a very small metaconule sometimes not completely differentiated from the former, and a heavy posterior cingulum, which terminates against the protocone without any suggestion of a hypocone. The second Princeton specimen just referred to (No. 12544) showing this type of third molar is also from the lower zone



12624

FIG. 2. *Archæotherium mortoni*, No. 12624 Princeton University Geological Museum. Right ramus of lower jaw from the outer side, about one fifth natural size. The anterior teeth are indicated by dotted outlines to localize the alveoles.

of rusty nodules of the Lower Oreodon beds, but lacks the lower jaw. In the collection of the American Museum of Natural History a very small skull, No. 1481, from the Oreodon beds on Hat Creek, Nebraska, shows an extreme degree of crowding of the lower anterior premolars (c–p_1, 4 to 5 mm.; p_1–p_2, 2.5 mm. to 3 mm.; p_2–p_3, 3 mm. to contact) associated with a type of m^3 indistinguishable from that just described. In a partial skull without lower jaws in the same collection, from the Middle Titanotherium beds on Lance Creek, Wyoming, the third upper molar (Fig. 5), which here has a slight external cingulum, shows even greater reduction of the cuspidation in the posterior row where the posterior intermediate is absent and metacone and posterior cingulum blend.

It is evident from the above that small forms with reduced m^3, in some of which the lower anterior premolars are closely crowded, have a wide range both in space and time, and it will, accordingly, be advisable to look into the matter of their relationship with both *Archæotherium coarctatum* and *A. mortoni*. All of the specimens to which I have referred are perfectly typical *Archæotherium*, differing from *Entelodon* in the triangular crown and notched anterior border of the fourth upper premolar, the spacing between the upper premolars anterior to p^3, the absence of greater width in the posterior part of the crown of p^3 and the presence of distinct para- and metaconids in the lower molars, where preserved in association with the skull. In view of the comparisons to follow it is a fair presumption that the Cypress Hills form, from its resemblance to the South Dakota and Nebraska skulls in the matter of crowded lower premolar dentition and other features, should properly be referred to *Archæotherium*, rather than to *Entelodon*.

A. coarctatum is based on a left mandibular ramus with all the teeth except incisors and canine, and the following characters are either specified by Cope or may be deduced from his figure.[3] Comparison will be made with the newly discovered material, with crowded lower premolar dentition, as we proceed.

1. *A. coarctatum* differs from all other small entelodonts so far described in the absence of diastemata between the lower premolars, except for a very short one between the first and second, resembling the Bear Creek specimen in this respect, where the diastema is somewhat longer, due to the greater size of the individual and differing from the Hat Creek skull (No. 1481, Am. Mus.) in lacking the small inconstant spacing between p_2 and p_3.

2. The first lower premolar is separated by a very short space from the canine, also true of these specimens.

3. The third premolar is larger than the fourth and the first and second are abruptly smaller than either of the others. P_3 was certainly the largest in the Bear Creek specimen (Princeton 12624), with p_2 not much shorter than p_4, judging from the extent of the empty alveoles. In the skull from Hat Creek the teeth have proportions

[3] Loc. cit., Pl. XIV., Figs. 3, 3a.

comparable to those shown in Fig. 4 *B* (see table of measurements) and are slender and compressed laterally.

4. P_1 is said to have a compressed laterally-grooved single root in *A. coarctatum*. There are alveoli for two large roots in No. 12624 (Fig. 4 *A*) and the small p_1 of the Hat Creek specimen is double-rooted.

5. The lower molars of *A. coarctatum* have the anterior tubercles elevated above the posterior row. The same is true in both the Bear Creek and Hat Creek specimens, but not more so than in some individuals of typical *A. mortoni*.

6. Heels in lower molars with three tubercles, the third or posterior median of which is said to be better developed than in *A. mortoni*, especially on the last molar. Cope's figure shows the heel of m_2 to be as wide as the trigonid, with hypoconid and entoconid of the same size, while in m_3 it is narrower, with three large cusps, the hypoconid and entoconid of the same size and the hypoconulid very large. In our Princeton specimen from Bear Creek the heels of both m_2 and m_3 are narrower than the trigonids, with the hypoconid the largest and most prominent of the heel cusps, the other two being distinct but much smaller (Fig. 4 *A*). In the Hat Creek specimen (No. 1481, Am. Mus.) the heel of m_2 is less narrow transversely than in the Princeton specimen, but the hypoconid is still the largest cusp, as it is also in m_3, where the entoconid on the right side is somewhat larger than in the Princeton specimen and of about the same proportions as in it on the opposite side.

7. No internal cingulum is present on the molars of *A. coarctatum* and the enamel is smooth. There are no internal cingula in the specimen from Bear Creek and the enamel is smooth on the triturating surface of the crown and slightly rugose on the sides. In the Hat Creek specimen it is practically smooth.

8. Anterior border of coronoid sloping backward. Notch between coronoid and condyle more deeply concave than in the Bear Creek specimen, where the front edge of the coronoid rises vertically, as it does also in No. 1481 from Hat Creek.

9. Lower premolar crowns strongly compressed laterally, especially the anterior ones. None are preserved with our No. 12624, but

in No. 1481, Am. Mus., they are as strongly compressed laterally as in our No. 11440 (Fig. 4 *B*), a typical *A. mortoni* as noted later.

Certain unassociated upper molars from the Cypress Hills locality are referred to *A. coarctatum* by Cope and Lambe.[4] Of these, the third molar, as figured by Lambe, has three well-developed and subequal cusps in the posterior row and strong external cingulum, quite different from the less complex type of cuspidation found in the specimens just described. Whether this specific reference is correct

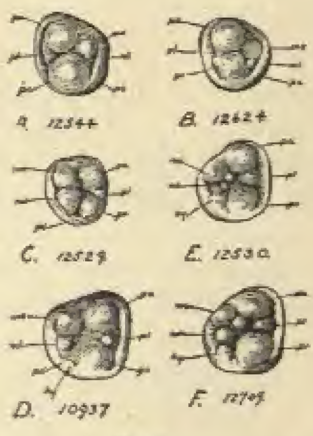


Fig. 3. *Archæotherium mortoni.* Series of crown views, from contemporary animals, of the third upper molar, one half the natural size, showing some of the variations in cusp development. *A* and *B* are of the left side; the others are of the right side. *C* is drawn from a tooth not fully erupted, the anterior cingulum being still covered by bone. *pr*, protocone; *pl*, protoconule; *pa*, paracone; *me*, metacone; *ml*, metaconule; *pr*, posterior cingulum; *hy*, hypocone.

can not be determined until lower jaws of unquestioned *coarctatum* type are found in association with upper molars.

The skulls from Hat Creek and Bear Creek differ greatly in size (see table of measurements) and the latter has been deformed somewhat by crushing, and has, unfortunately, lost the tip of the cheek

[4] L. M. Lambe, "The Vertebrata of the Oligocene of the Cypress Hills, Saskatchewan," *Contributions to Canadian Palæontology*, Vol. III., Part IV., p. 26, Pl. II., Figs. 10, 11, 1908.

flange and the dependent mandibular processes. The outward and backward slope of the cheek flange (Fig. 1 *A*) is undoubtedly accentuated by crushing, but its width has not been increased thereby. Apart from its greater size, the part remaining is not essentially unlike that of the small skull in the American Museum collection (Hat Creek specimen), which, in turn, except for the reduced m^3, can not be distinguished from our No. 12709 shown in Fig. 1 *B* and regarded as typical *A. mortoni*.

The type of *A. coarctatum* lacks the inferior mandibular border anterior to the "first" (fourth) premolar, consequently the shape of the dependent processes is unknown. Both processes are lost by decay in No. 12624 (Fig. 2), but were evidently present. Fortunately, they are well preserved in the Hat Creek specimen (Am. Mus., No. 1481), where they are seen to be of the *A. mortoni* type, made familiar by Peterson's drawing.[5]

Turning now to *A. mortoni*, the type specimen, a fragment of the maxillary with p^3 and p^4 in place, figured by Leidy on Plate IX., Fig. 3, "Ancient Fauna of Nebraska," is manifestly inadequate from the modern point of view, but we must not forget that Leidy was just as fully entitled to find his own original type inadequate as any subsequent writer and, therefore, to redefine it in terms of other specimens. The young individual with milk and permanent dentition which he figures on Plates VIII. and IX. of the Ancient Fauna is, therefore, an "heutotype," as this term is defined by Schuchert and Buckman.[6] On this specimen, I submit, the species *Archæothèrium mortoni* is adequately founded. It is an individual with richly tuberculated third upper molar, with three cusps in the front row and a large number of small tubercles in the posterior row among which the metacone, metaconule, hypocone, posterior cingulum and an additional cusp anterior to the metacone and metaconule may be made out, although by no means as distinctly as in Fig. 3 *E* and *F*. There may be departures from this fully tuberculated type of m^3 in the development of the hypocone which may be barely distinguishable from the posterior cingulum (Fig. 3 *D*) or large and fully formed (Fig. 3

[5] Loc. cit., p. 48, Fig. 4.

[6] *Science*, N. S., Vol. 21, No. 545, p. 900, 1905.

E, F), and also a variation in the size of the metaconule (Fig. 3 *C–F*). Lower jaws are associated with some of the skulls showing these types of third molar and in them variations in the spacing of the anterior premolars and in the structure of the lower molar heels

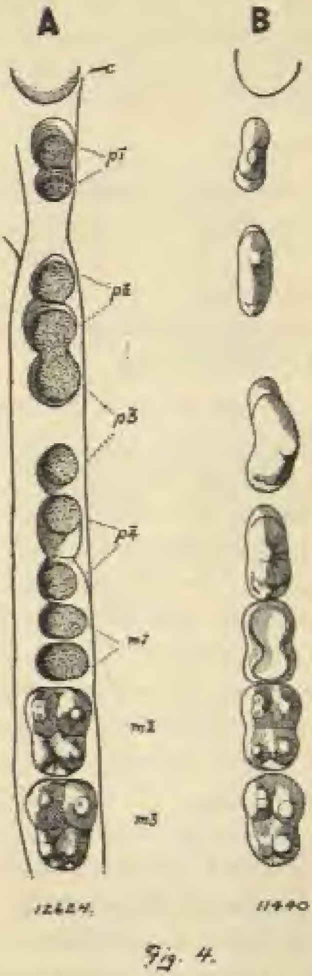


FIG. 4. *Archæotherium mortoni*, lower dentition of the right side crown view, one half of the natural size. *A*. Lower jaw of large skull from Bear Creek, showing the empty alveoles of the anterior teeth, m_2 and m_3 alone remaining in place. *B*. Lower dentition with well-spaced premolars belonging with a small skull from Sand Creek, Nebraska. Both specimens in the Princeton collection.

may be noted. Princeton No. 11440 is a typical *A. mortoni* with a large hypocone on m^3 widely spaced anterior lower premolars and a large hypoconulid and entoconid in the heel of m_3 (Fig. 4 *B*). No. 1483, American Museum, from the Oreodon beds on Hat Creek, Nebraska, has the hypocone of m^3 intermediate in size between that shown in Fig. 3 *D* and 3 *F*, with the spacing between the lower premolars as follows: c–p_1, 5 to 7.5 mm.; p_1–p_2, 11.5 to 13 mm.; p_2–p_3, 3.5 to 4 mm.; p_3–p_4, 2 mm. to contact, in contrast with c–p_1, 6; p_1–p_2, 9; p_2–p_3, 17; p_3–p_4, 3.5, as shown in Fig. 4 *B*. In this second Hat Creek specimen the heel of m_3 is similar to that of our No. 12624 (Fig. 4 *A*), except that the hypoconulid is a broad ledge instead of being broken up into accessory cusps. In Peterson's figure[7] the spacing of the lower premolars is as shown in our Fig. 4 *B*, the hypocone is small and but slightly differentiated from the posterior cingulum in m^3, and the third lower molar has a heel similar to that shown in Fig. 4 *A*, except for the minor cuspidation there seen. The longest gap in lower premolar spacing is ordinarily between p_2 and p_3 as in Fig. 4 *B*, but in No. 1483, American Museum, reference to the dimensions just given will show that it is between p_1 and p_2. All the variations in molar pattern shown in Fig. 3 are strictly contemporaneous, all the specimens being from the lower zone of rusty nodules of the lower Oreodon beds. Whether the development of cingula, rugose enamel and accessory cuspules in the teeth of entelodonts is to any extent nutritionally controlled, as suggested by Nathusius's[8] observations on domesticated pigs, is difficult to determine, but may well be used with caution in establishing or defining species.

We may next turn to the specimens in the Yale collection which Mr. Troxell has named *A. clavus clavus* and *A. clavus darbyi*. Mr. Peterson, in his monograph on *Dinohyus*,[9] has regarded the first as a subspecies of *A. mortoni*, and I have elsewhere indicated my inability to distinguish between them and specimens in the Princeton collection referable to *A. mortoni*. The material secured by our 1921 Expedition rather increases the difficulty in that it presents additional types

[7] Loc. cit., pp. 48, 49, Figs. 4–6.

[8] Vorstudien für Geschichte und Zucht der Hausthiere Zunaechst am Schweineshaedel. H. von Nathusius, Berlin, 1864.

[9] Loc. cit., p. 49.

of molar pattern and premolar spacing in association with skull structures like those of Mr. Troxell's *clavus* and *darbyi*, which, in turn, are not essentially unlike those of *A. mortoni*. An examination of Mr. Troxell's text and Fig. 3[10] will show that m^3 of *A. clavus clavus* is of much the same general type which I have attempted to represent in Fig. 3 *D*. Mr. Troxell describes it as follows: "The protocone has a position midway fore and aft, and has encroached on the hypocone so that the latter is scarcely visible; it is, in fact, smaller than the metaconule and is nothing more than a heel, continuous with the hypostyle. The metacone is also lower than the metaconule, which already shows a lake of dentine by reason of the wear." In the inferior dentition, in m_2 "the hypoconid is strongly developed; on the other hand, the entoconid is weak. A distinct posterior heel may be seen. M_3 is much like m_2, except that a very marked posterior heel and a less strong hypoconid are observed," and these points are well brought out in the drawings accompanying Mr. Troxell's text. In

Fig 5.

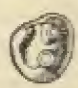


FIG. 5. *Archæotherium mortoni*, No. 9315 American Museum. Right upper m^3 showing an extreme degree of cusp reduction. One half the natural size.

A. clavus darbyi m^3 is "round or slightly oval, due to the strong lobes of the para- and hypocones. There is a moderate cingulum anterior only and no heel. The protocone is forward of the mid-line. The hypocone is strong." In Fig. 8 of Mr. Troxell's paper, where the lower teeth, with somewhat worn crowns, are represented, there is a weak entoconid on m_2; no hypoconulid is shown on this tooth, while on m_3 there is a very large hypoconid and a posterior and internal ledge from which the remaining cusps do not seem well differentiated. Perhaps this is what Mr. Troxell refers to when he writes of "the absence of heel in m_3" being an unusual feature which distinguishes it "from the other specimens present." Neither of the Yale specimens possess unspaced lower premolars.

In the presence of this melange of characters I am quite unable to separate species on the basis of constant association of constant differences. Certain characters, like the complication of crown pat-

[10] Loc. cit., pp. 364, 365.

tern in m^3, may be regarded as progressive, but among contemporary individuals in the series the increments of change appear to fluctuate in an irregular way, certain cusps in less advanced teeth being very large, while in more advanced teeth they may be quite small (compare m^1, Fig. 3 *C, E*), or small in some and large in others (m^1, Fig. 3 *E, F*). Small bodily size is not confined to individuals with less complex m^3 or those with the anterior lower premolars unspaced, and these are connected with the more richly cuspidated and fully spaced types by such a transitional form as No. 1483, Am. Mus., where m^3 resembles Fig. 3 *D*, and there are very short spaces between the anterior lower premolars. So far as the assumed primitiveness of *A. coarctatum* is concerned, every one of its characters which might be regarded as primitive is possessed in some degree by specimens which differ from it in other respects, as I have tried to show, and we are faced by two alternatives, either the naming of every variant, which results in making practically every specimen a separate species, for almost every one of them shows a new grouping of characters which appear somewhere else in a different association, or the referring of the lot to one species for which the name *A. mortoni* has priority and which seems, as Dr. Matthew has suggested to me in another connection, to be made up of several interbreeding strains, diagrammatically a number of anastomosing lines, which differ by various small unit characters or combinations thereof, transmitted to the individual from the various pure lines which enter into its ancestry. Reference to the accompanying table of measurements will show a considerable range in size, but I am not able to correlate this, as already indicated, with the structural variations noted above.

The situation with regard to these small entelodonts suggests that certain of the structural differences which have been used for the separation of some of the larger forms would be found to intergrade if we had larger series of contemporary specimens, which, unfortunately, do not yet exist in museum collections. If the conclusions reached above are well founded, it may be considered certain that *A. mortoni* ranges down into the Titanotherium beds,[11] but neither it

[11] Peterson reports a number of skulls, portions of skulls and teeth in the Carnegie Museum, collected by himself and others from the Titanotherium beds of Nebraska and South Dakota and agreeing with *A. mortoni* as figured by Leidy. Peterson, loc. cit., p. 47.

nor the genus to which it belongs has yet been reported from the Protoceras-Leptauchenia level.

	Measurements. Oreodon Beds.						Titanotherium Beds.	
	Am. Mus. No. 1481	P. No. 12530	P. No. 11440	P. No. 12709	Am. Mus. No. 1483	P. No. 11529	P. No. 12624	Am. Mus. No. 9315
Skull length, ant. base of canine to condyle (inclusive)	323	334		343	377	378	423	
p^1–m^3, length	147	173	178	183	181	192	185.5	
m^1–m^3, "	59	69	65.5	78	69.5	67+	65	
p_1–m_3, "	160	—	197	—	188	—	200	
m_1–m_3, "	63	—	71	—	74	—	72	
p^4 ant. post.	18.5	19.5	18.5	22	20	22.5	19	18.25
" transverse	18.5	20	19	22	22.5	21.3	19.5	20
m^1 ant. post.	21	22	20.5	23	21	24	21.25	19
" transverse	22	21	20	23	24±	23	19	19
m^2 ant. post.	21	23	23.5	26.5	23.5	26	23	22.5
" transverse	23.5	23.5	23	25	—	28	22	21
m^3 ant. post.	18	21	21	23	23		18 18.5	17 18
" transverse	18.5	20.5	21	22	24			
m_1 ant. post.	20		22		22			
" transverse	14.5		13		15			
m_2 ant. post.	21.75		23.5		26			
" transverse	16		16		—			
m_3 ant. post.	21.75		24.5		25.5			
" transverse	16.5		15.5		19			

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HYRACODONS FROM THE BIG BADLANDS OF SOUTH DAKOTA.

Investigation aided by a grant from the Marsh Fund of the National Academy of Sciences.

By WILLIAM J. SINCLAIR.

(*Read April 22, 1922.*)

Introduction.

Studies in progress at Princeton on the *Mesohippus bairdii-Oreodon culbertsoni* zone of the White River Oligocene, typically developed in the Big Badlands of South Dakota, have made necessary an examination of specific characters in the genus *Hyracodon*, considered as an index fossil, and also a review, in this connection, of a recent paper on the subject by Mr. Troxell.[1]

The Four Specific Types.

Four distinct types can easily be recognized as follows:

A. With the end of the protoloph curving round the end of the metaloph in p^4 and completely fusing therewith in worn teeth, isolating a central depression. Inner wall of tooth not deeply grooved. Anterior cross-crest in p^1 present, but may be small and little more prominent than the cingulum. Fig. 1.

B. With the transverse valley of p^4 blocked by a spur from the protoloph which abuts against the anterior wall of the metaloph. The latter cross-crest is longer than the former in unworn teeth, but tends to shorten up as the tooth wears. Inner wall of tooth deeply indented; p^1 with anterior cross-crest present or absent. Fig. 2, *A–C*.

C. Transverse valley of p^4 wide open; p^1 with anterior cross-crest small or absent. Fig. 3, *A, B*.

D. Transverse valley of both p^4 and p^3 wide open. Anterior cross-crest in p^1 of variable size. Figs. 4, 5.

[1] "New Species of Hyracodon," *American Journal of Science*, II., July, 1921, pp. 34–40.

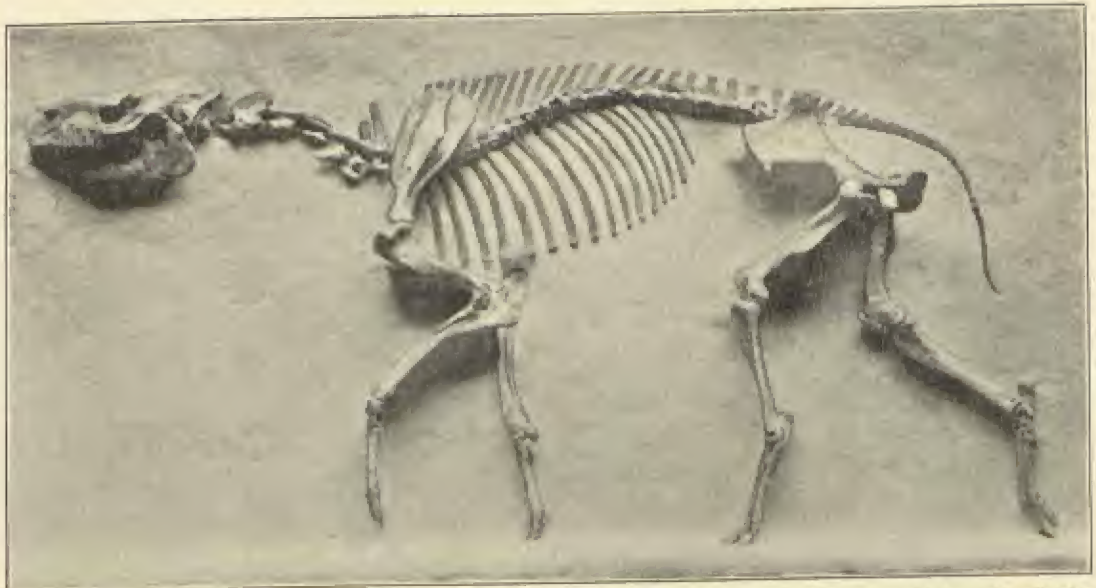

FIG. 8. The reconstructed skeleton of *Hyracodon apertus* sp. nov. Nos. 11414, 11415 Princeton University Geological Museum, standing 30¼ inches high at the shoulder and showing to advantage the cursorial adaptation of the animal.

I am unable to separate these four groups on the basis of the lower dentition. For systematic and stratigraphic purposes they may be conceived as species, although some might wish to term them subspecies. It will be noted that the distinctions between them are based primarily on structural differences in the upper posterior premolars which become increasingly molariform, the change beginning with the fourth and working forward, a situation occurring in many mammalian groups and always regarded as indicative of progressive evolution. No intermediate stages between these four types of structure in p^4 have been observed and, in the absence of blending, they are probably to be regarded as distinct species, on the basis of constant association of constant differences. Whether these slight differences in dental structure were accompanied by sexual antipathy between the various types is, of course, outside the realm both of palæontology and available data. It is possible that the four types were derived from each other in the order mentioned, ancestor and descendant continuing to exist contemporaneously for a time (see table showing vertical range). Other assumptions are equally possible.

In groups A, B and C there is a sequence of size variations, intergrading by small increments, so that size must be ruled out for purposes of specific discrimination unless we are content to change the name every few millimeters.

Nomenclature and Synonymy.

In specific nomenclature there exists some confusion which, I believe, can now be eliminated. There can be no question that the tooth structure described in A and illustrated in Fig. 1 is identical

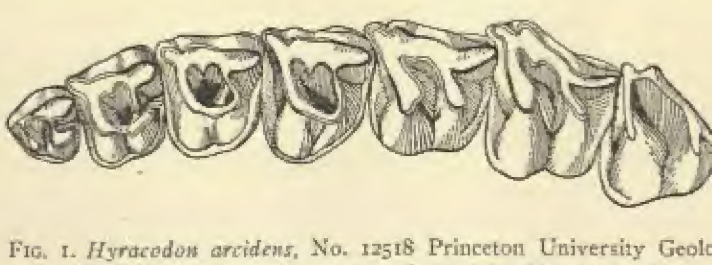

Fig. 1. *Hyracodon arcidens*, No. 12518 Princeton University Geological Museum. Upper premolar-molar series of the left side, crown view, three fourths the natural size.

with that shown in a specimen from the Titanotherium beds at Bone Coulee, Cypress Hills, Saskatchewan, and designated by Lambe[2] *Hyracodon priscidens*. Cope's[3] brief characterization of *Hyracodon arcidens* makes it evident that he had before him a specimen showing the same features as does our No. 12518 (Fig. 1) from the zone of rusty nodules, forty feet, more or less, above the base of the Oreodon beds in Indian Creek, Pennington County, South Dakota. Similarly, Troxell's *Hyracodon arcidens mimus*[4] from the Oligocene at Deadwood, South Dakota, and his *Hyracodon selenidens*[5] from the Middle Oligocene of Colorado are size variants within the same structural range. Cope's *Hyracodon arcidens* must take priority. The holotype of this species seems to be misplaced, as the specimen so recognized by the American Museum (No. 6309, Am. Mus. Nat. Hist.) and figured as the holotype in the Cope plates[6] does not agree with Cope's description of the specimen on which his species was based. The so-called holotype, from the Cedar Creek beds (Oreodon zone) of Logan County, Colorado, is an old individual with the crown patterns of the premolars obliterated by wear and is practically indeterminate specifically. *Hyracodon priscidens* Lambe, *H. arcidens mimus* Troxell and *H. selenidens* Troxell take precedence in the order given.

The various specimens of what we may now speak of as *Hyracodon arcidens*, when arranged in order of size, are seen to intergrade by small increments as follows:

Cope's type	premolar series, length 72 mm.[7]
Princeton Geol. Mus. No. 10816	premolar series, length 71.5
Princeton Geol. Mus. No. 12518	premolar series, length 67
H. arcidens mimus, Yale P. M. No. 11174	premolar series, length 67[8]
Lambe's type of *H. priscidens*	premolar series, length 57[9]
H. selenidens, Yale P. M. No. 11173	premolar series, length 57[8]

[2] L. M. Lambe, *Trans. Royal Society of Canada*, second series, 1905–6, Vol. XI., Section IV., pp. 37–42, Plate I, issued August, 1905.

[3] E. D. Cope, Paleontological Bulletin No. 15, p. 2, issued August 29, 1873.

[4] E. L. Troxell, *loc. cit.*, p. 36.

[5] Ibid., p. 37.

[6] Hitherto Unpublished Plates of Tertiary Mammalia and Permian Vertebrata. U. S. G. S. and *Am. Mus. Nat. Hist.*, 1915, Plate CII. I am told that this specimen was marked as the type on Cope's label which accompanied it.

There are comparatively few specimens available of this species. Additional ones would undoubtedly afford a larger series of size variants and close some of the gaps, but even those listed above are sufficient to show intergradations by less than ten per cent. in some cases

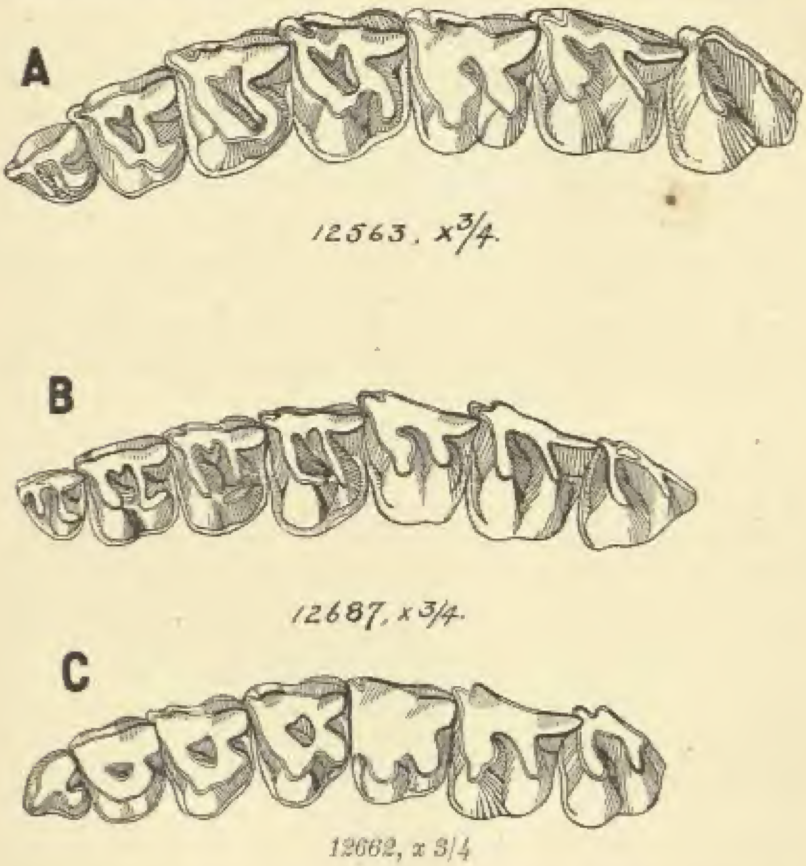

FIG. 2. *A*, maximum, *B*, intermediate and *C*, minimum size variants of *Hyracodon nebrascensis.* Upper premolar-molar series of the left side in crown view, three fourths the natural size. Specimens No. 12563, 12687, 12662 Princeton University Geological Museum.

[7] Measurements given by Cope in type description.

[8] Measurements given by Troxell, loc. cit., p. 40.

[9] Scaled from Lambe's "natural size" figure, loc. cit., Plate I. His table of measurements in the text (p. 40) gives 62 mm. for this dimension. If this is correct, there are intergradations of less than 10 per cent. throughout in the variation series listed above.

and a little under twenty per cent. in others.[9] In view of similar, but more complete, series of intermediates to be discussed in the paragraphs to follow, I feel that we are here dealing with individual variations and recognize but one species possessing the characters described under A and shown in Fig. 1, which, I suggest, we agree to call *Hyracodon arcidens* Cope.

Turning now to the tooth structure described under B and shown in Fig. 2 *A–C*, to specimens showing which I believe Leidy's name *Hyracodon nebrascensis* is applicable, it is quite true, as Mr. Troxell points out, that the original description was without figures (many of them were at that date), and that the drawings accompanying Leidy's memoir on "The Ancient Fauna of Nebraska" show, as Mr. Troxell observes, "a widely diversified group." While Dr. Leidy did not always designate his early types in subsequent publications dealing with them, he frequently figured them there carefully, and, in this particular case, his reference to the type specimen is sufficiently detailed to make its identification from the figures practically certain, as will appear from the following considerations:

Mr. J. W. Gidley has kindly placed at my disposal a so-called "type" of *Hyracodon nebrascensis* preserved in the United States National Museum (No. 138 of their vertebrate palæontological collection). This is the specimen figured on Plate XII*A*, Fig. 6, of Leidy's "Description of the Remains of Extinct Mammalia and Chelonia" from D. D. Owen's "Report of a Geological Survey of Wisconsin, Iowa and Minnesota; and Incidentally of a Portion of Nebraska Territory," Philadelphia, 1852. Comparison of the specimen with Fig. 13 of Plate XIV., "Ancient Fauna," will show that it is the same as the one there figured, for the artist has faithfully rendered certain minor breaks in the teeth which make identification unquestionable, but the drawing, as reproduced, is a little smaller than the natural size. In the first of the publications just referred to, Leidy states that "this species was first established upon the anterior portion of a skull and lower jaw, containing all the molar teeth of an old individual belonging to the collection of the Smithsonian Institution." He then goes on to mention two specimens in Dr. Owen's collections (which are again referred to in the text of the "Ancient Fauna," p. 86), "a head of the same species, of a very old

individual, with the upper part of the whole length broken away," which "contains all the molars nearly perfect," with "the crowns worn nearly to the edge of the alveoli," and "also in the same collection a face very much mutilated, except the forehead, of an individual which had just reached adult age" and which "contains all the molars nearly perfect, the last one about two thirds protruded." This second-mentioned specimen is the so-called "type," beyond the peradventure of a doubt, for it shows all the features enumerated by Leidy, and has been figured by him, as I have indicated, and as he states himself, in Fig. 13, Plate XIV., "Ancient Fauna."[10] The very old individual with the top of the head gone is, also according to Leidy's own statement, the original of Plate XV., Figs. 1 and 2, "Ancient Fauna."[11] On pages 86 and 87 of this memoir a list of all specimens of *Hyracodon* known up to the date of acceptance of the memoir (December, 1852) is given. Omitting all the references to fragments which, manifestly, have no connection with our endeavor to identify the type, and passing over the descriptions of the two Owen specimens, which appears in substantially the same words as have been quoted above, we find at the head of the list mention made of "the anterior portion of a skull, accompanied by the lower jaw, of an adult individual. The former has the forehead, orbital entrance, and molar teeth well preserved, but the face is very much broken and its nasal part is displaced. The lower jaw contains all the molars in perfect condition, but it has lost its rami and the symphysis." These parts are figured on Plate XIV., Figs. 1–3, and on Plate XV., Fig. 3, of the "Ancient Fauna" and are said to be from Captain Stewart Van Vliet's collection. Nothing is said about the type in the Smithsonian Institution which he had previously described in similar phrase, speaking of it as an old individual, which a glance at the plates will show the Van Vliet specimen to be. I do not regard this as an oversight, but believe that Leidy had in mind one and the same specimen, which might very well have come from Captain Van Vliet's collection and yet belong to the Smithsonian Institution.[12] Leidy's figure shows

[10] See "Ancient Fauna," page 86, third line from foot of page.

[11] See page 87, second line.

[12] Mr. Gidley is unable to supply any information regarding the present location of this specimen. No. 138 of the National Museum collection, according to Mr. Gidley, is recorded in the old catalogue as having been collected by Dr. John Evans.

an individual with well-worn teeth, but with a posterior premolar of the blocked-valley type, with deeply grooved inner tooth wall as defined under B, and of a size practically identical with No. 12687 (Fig. 2 *B*) of the Princeton collection. I, therefore, submit that Leidy's *Hyracodon nebrascensis*, in this limited sense, is entirely applicable to that sequence of individuals within the genus which has the transverse valley of p^4 blocked in the manner indicated, and that no adequate grounds exist, or have existed, for discarding the name. Figures 4–8, Plate XIV., of the "Ancient Fauna" seem to represent unworn examples of the same type.

As in the preceding species, a series of individual variants can be made out, intergrading by increments of not more than four millimeters between extremes in the length of the molar series, here taken as the basis for comparison, because present completely in all the specimens studied for dimensions. Material from the Princeton collection only is used as follows:

No. 12662, molar series,	length 57
No. 12687, molar series,	length 61
No. 12688, molar series,	length 61
No. 12666, molar series,	length 65
No. 12680, molar series,	length 67.5
No. 12563, molar series,	length 71
No. 10723, molar series,	length 72

Of these, the first five are contemporary and from a six-inch zone of rusty nodules about sixty feet (sometimes less) above the top of a similar zone affording No. 12563, which, in turn, lies some forty feet, more or less, above the base of the Oreodon beds and constitutes the lower zone of rusty nodules of our Princeton field nomenclature, while the other may be designated as the upper rusty nodular zone of the Lower Oreodon beds. No. 10723, on the other hand, is from well up in the Protoceras-Leptauchenia beds of the Upper Oligocene and is but a trifle larger than its Lower Oreodon beds predecessor. The specimen numbered 12662 (Fig. 2, *C*) is as small as Mr. Troxell's *H. selenidens*, which I regard as an individual variant of *H. arcidens*, as already indicated, for exactly the same reasons which induce me to place this small specimen of *H. nebrascensis* as a terminal size variant in the *nebrascensis* series. Each conforms to its own structural type, but intergrades by small-size increments with the largest specimens referable thereto.

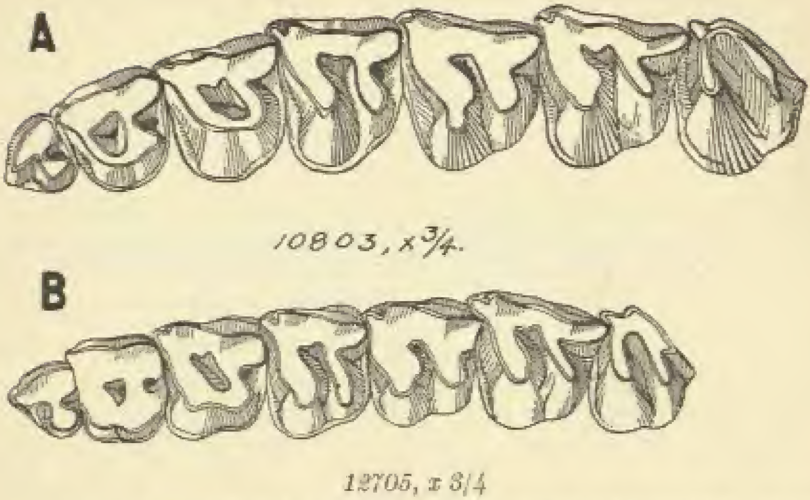

FIG. 3. *A*, maximum and *B*, minimum size variants of *Hyracodon apertus* sp. nov., of which No. 10803 is the type. Upper premolar-molar series of the left side in crown view, three fourths the natural size. No. 10803, 12705 Princeton University Geological Museum.

For the species described as C we are without a name and I propose that it be known as *Hyracodon apertus* sp. nov., with reference to the wide-open valley of p^4 as described under C above, designating as type a skull with right ramus of the lower jaw, No. 10803, Princeton University Geological Museum, collected by Mr. J. B. Hatcher in 1893 from a zone of brown nodules above the Protoceras sandstones in Corral Draw, South Dakota. A crown view of the premolar-molar series is given in Fig. 3 *A*. Regrettable as the proposing of additional specific names may be, it is unavoidable in this instance in order to designate a form already adequately figured by Leidy in Fig. 13 on Plate XIV., "Ancient Fauna," but grouped by him with *nebrascensis*, from which, however, the absence of blocking in the valley of p^4 easily separates it.[13] To this species belongs the mounted

[13] Long-continued wear would, undoubtedly, isolate a central depression as shown in Leidy's figure of *nebrascensis* ("Ancient Fauna," Pl. XV., Fig. 3) or in Fig. 2 *C*. At corresponding stages of wear, however, p^4 is unblocked in *apertus* and completely blocked in *nebrascensis* (compare Figs. 2*A* and 3*A*). The development of a transverse spur from the protoloph would convert p^4 of *apertus* into a tooth of the *nebrascensis* type; its loss in *nebrascensis* would result in the structure found in *apertus*.

skeleton in the Princeton University Geological Museum (Nos. 11414, 11415, Fig. 8) monographed some years ago by Professor Scott[14] as *Hyracodon nebrascensis*.

Here also a series of individual variants may be arranged, using the length of the molar series as our basis for comparison:

Princeton Geol. Mus. No. 12705	molar series, length 59
Princeon Geol. Mus. No. 12702	molar series, length 64
Princeton Geol. Mus. No. 11414	molar series, length 64
U. S. National Mus. No. 138	molar series, length 65[15]
Princeton Geol. Mus. No. 10803	molar series, length 72.5

The horizon of Leidy's specimen is unknown. Of the others, No. 12705 is from the lower zone of rusty nodules of the Oreodon beds, No. 12702 from the upper nodular zone of the same, No. 11414 from the Upper Oreodon beds, and No. 10803, the type from the Protoceras-Leptauchenia beds of the Upper Oligocene. Once more I am convinced that these are only individual variations, and that, while there is a gap of a little over ten per cent. between the last two members of the series, additional specimens, when available, will close it.

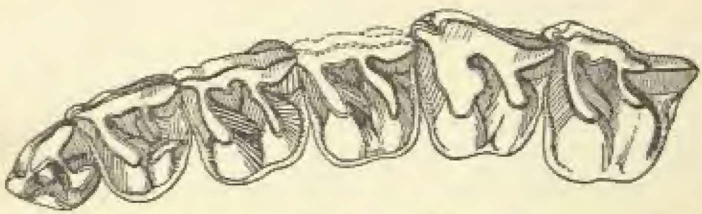

FIG. 4. *Hyracodon leidyanus*, No. 10802 Princeton University Geological Museum. Upper premolar-molar series of the left side, crown view, three fourths the natural size. The tip of m[3] appears just above the gum and is not shown in the drawing.

To the fourth species, described under D, Mr. Troxell's name, *Hyracodon leidyanus*, seems entirely applicable. In Fig. 4 is shown the upper premolar-molar series of an incomplete skull and lower jaws, No. 10802 of the Princeton collection, from the rusty nodular layer above the Protoceras sandstones in Corral Draw, in which the

[14] W. B. Scott, "Die Osteologie von Hyracodon Leidy," *Festschrift für Carl Gegenbaur*, Leipzig, 1896. Pp. 353–384. Three plates.

[15] In Fig. 13, Pl. XIV., "Ancient Fauna," this dimension is 62 mm., showing that the figure is not quite natural size.

third molar is just appearing above the gum. In Fig. 5 a very much older individual (No. 10144, Princeton University Geological Museum) is seen, in which the open valley is retained in both the third and fourth premolars, both of which are well worn, as are also the anterior molars. The third molar is fully erupted and partly worn. Both these specimens are fractured in such a way as to show the absence of germs of replacement teeth above the posterior premolars and I can not escape the conclusion that we are here dealing with per-

FIG. 5. *Hyracodon leidyanus*, No. 10144 Princeton University Geological Museum. Upper premolar-molar series from p^3–m^3 of the right side, crown view, three fourths the natural size.

manent teeth. In *Hyracodon* the milk dentition is more advanced in crown-pattern development than are the permanent premolars, even milk p^2 having an unblocked transverse valley (Figs. 6, 7). These molariform milk teeth have lower crowns and thinner enamel than their successors, and in p^2 there is a prominent style from the cingulum at the entrance of the transverse valley. Mr. Gidley has called my attention to certain specimens in his charge in which no germ teeth of the replacement series appear beneath the milk premolars and suggests that they "were very late in forming, but when started grew very rapidly to replace the milk dentition which seems sometimes to have persisted until all the true molars have come into use."[14]

FIG. 6. *Hyracodon* sp., No. 12679 Princeton University Geological Museum. Milk dentition and m^1 and ^{2} of the left side, crown view, three fourths the natural size. M^3 had germinated but is not preserved with the other teeth. Germs of the replacement series appear in the maxillary above the first and third milk premolars.

[14] Personal letter.

This is probable, as shown by our No. 12679, but I do not feel justified in regarding the well-worn premolars of our No. 10144 as persistent milk teeth and so have accepted without question Mr. Troxell's species.[17] Some doubt seems to exist about the stratigraphic position of his type specimen which is stated to be from the Middle or Lower Oligocene, Crow Buttes, South Dakota. The first-mentioned Princeton specimen occupies the stratigraphic position which might be expected for such an advanced form, which, so far as anyone can say at present, may range into lower beds and even occur in the Lower Oligocene, a question which can be settled only by further collecting. The second Princeton specimen was obtained by the Expedition of 1882 and is from the White River Oligocene of South Dakota, occurring in a matrix resembling that of the Leptauchenia beds, but no further information is available regarding its stratigraphic position. As there are but three specimens of *Hyracodon leidyanus* so far available, which agree closely in dimensions, no series of size variants can be recognized.

FIG. 7. *Hyracodon* sp. No. 3758 United States National Museum. First to third milk premolars of the right side, crown view, three fourths the natural size. No germs of the replacement series have formed above these teeth

Of the other species discussed by Mr. Troxell, "*Hyracodon*" *planiceps*[18] appears among the rhinocerotidæ in Dr. Matthew's faunal

[17] The milk dentition shown in Fig. 6 has the crown-patterns of the premolars more obliterated by wear than in m 1 and the necks of these teeth are farther below (*i.e.*, ventral to) the alveolar border than is true of the first molar, showing that they were erupted and in use before the appearance of that tooth. In Nos. 10802 and 10144 (Figs. 4 and 5), the first molar is abraded to a greater degree than the two preceding premolars and the necks of the tooth crowns are approximately on the same level, demonstrating that the premolars appeared after the eruption of m 1 and are of the replacement series. Mr. Troxell's drawing of *H. leidyanus* shows m 1 more extensively abraded than the premolars, so that their character as permanent teeth is fully established. Dr. Matthew has kindly called my attention to these criteria.

[18] W. D. Matthew, "Cenozoic Mammal Horizons of Western North America," Bulletin 361, U. S. G. S., in collaboration with Professor Osborn, p. 105, 1909.

lists published as far back as 1909. *Hyracodon major*, if properly referable to the genus, is specifically indeterminate and should be abandoned. The type consists of some skeletal elements in the Princeton collection, No. 10001, of unknown stratigraphic position other than that they are from the White River Oligocene of Dakota, and may be a little larger than the corresponding skeletal parts of the largest individuals of certain of the species here recognized. It is possible that still larger individuals of these existed than have so far appeared in the collections, but the diagnostic characters for their specific determination must depend, in any event, on the upper premolars and not on the skeleton.

This completes a survey of the nomenclature and synonymy of the genus to date.

Range in Time.

The following table shows the range of species in time and includes several specimens from the American Museum collection, not referred to in the text, kindly placed at my disposal by Professor Osborn and Dr. Matthew. It will be seen at once that each one of the four species recognized is not confined to a definite zone, but overlaps widely, in turn, on the time limits of the next and more advanced type. If Mr. Troxell is correct in placing *H. leidyanus* as low as the Titanotherium beds, it may well be that the divergence of all four species took place there, or even earlier, before the appearance of *Hyracodon* in the North American non-marine Oligocene of the plains region, to which it is, so far, narrowly confined and of which it is an excellent index fossil. For zonal purposes the species are of little value, as they range through a considerable thickness of beds, and the same is also true of the size variants.

In this table the heavy black lines show the position of the lower and upper zones of rusty nodules in the Lower Oreodon beds, both of which are properly included in the *Mesohippus bairdii-Oreodon culbertsoni* zone, the upper and lower limits of which have not yet been established. The specimens discussed in the table are from the following localities:

Hyracodon arcidens Cope.

8807 Am. Mus. Nat. Hist. Oreodon beds, probably upper according to Dr. Matthew, Logan Co., Colorado.

Species.	A. *H. arcidens* Cope.	B. *H. nebrascensis* Leidy.	C. *H. apertus* sp. nov.	D. *H. leidyanus* Troxell.
Protoceras-Leptauchenia Beds		P. 10723	P. 10803 8811 Am. Mus. Nat. Hist. (almost identical in size with P. 12705)	P. 10802
Upper Oreodon Beds	8807 Am. Mus. Nat. Hist. (exact size of P. 12518)		P. 10978 P. 11414	
Lower Oreodon Beds — *Mesohippus bairdii*–*Oreodon culbertsoni* zone, limits not yet established; 60′ or less; 40′ ±		P. 12666, 12680, 12687, 12688 P. 12662	P. 12702	
	P. 12518	P. 12563	P. 12705	
Titanotherium Beds	Lambe's Type of *H. priscidens* = *H. arcidens* Cope			

12518 Princeton. Lower zone of rusty nodules, Lower Oreodon beds, Indian Creek, Pennington Co., South Dakota.

Lambe's type of *H. priscidens.* Titanotherium beds, Bone Coulee, Cypress Hills, Assiniboia.

Hyracodon nebrascensis Leidy.

10723 Princeton. Brown nodular layer above upper sandstones of Protoceras beds, Corral Draw, South Dakota.

12662 Princeton. Fifteen feet below upper zone of rusty nodules, Lower Oreodon beds, Corral Draw, South Dakota.

12666, 12687, 12688, 12680 Princeton. Upper zone of rusty nodules, Lower Oreodon beds, Quinn Draw, South Dakota.

12563 Princeton. Lower zone of rusty nodules, east part of Indian Creek basin, Pennington Co., South Dakota.

Hyracodon apertus sp. nov.

10803 Princeton. Brown nodules above the Protoceras sandstone, Corral Draw, South Dakota.

10978 Princeton. Upper Oreodon beds, Quinn Draw, South Dakota.

11414 Princeton. Upper Oreodon beds, South Dakota.

12702 Princeton. Upper zone of rusty nodules, Lower Oreodon beds, Quinn Draw, South Dakota.

12705 Princeton. Lower zone of rusty nodules, Lower Oreodon beds, Corral Draw, South Dakota.

Hyracodon leidyanus Troxell.

10802 Princeton. Brown nodule layer above Protoceras sandstones, Corral Draw, South Dakota.

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THE USE OF DEVICES FOR INDICATING VOWEL LENGTH IN LATIN.

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The Latin language, like some others, passed through various stages as regards pronunciation and accent. Before the days of Livius Andronicus, as is shown by sundry well-known linguistic phenomena, there was a stress accent on the initial syllable of every word, accompanied in the longer words by a secondary accent. In a few instances this early accent has left traces in the literature of the ante-classical period; for example, in the versification of Plautus. In general, however, it had given place to one which fell on the penult or the antepenult, very rarely and for special reasons on the last syllable. The position of this new accent conformed to a simple rule, falling on the penult if that syllable was long; otherwise on the antepenult. At about the same time greater importance was given to the quantity of vowels and syllables, not only those whose quantity determined the accent, but throughout all words. This careful observance of quantity seems to have been due to Greek influence, which made itself felt first in Roman versification and then in the speech of educated Romans.

It is well known that the meters used by the Roman poets, except for the occasional survival of the native Saturnian, were borrowed or adapted from those of the Greeks, who based their metrical schemes upon quantity. A dactyl, for instance, consisted of a long syllable followed by two shorts; not, as in English, of a stressed syllable followed by two without stress. That Latin verse, for some centuries after the time of Livius Andronicus, was based upon quantity is universally recognized. It is also believed by most scholars, whatever their actual practice may be, that there was no "ictus" in the form of a strong stress on the first syllable, for example, of a dactyl or spondee, as in English and as in Latin poetry as it was formerly read, or "scanned," by the English-speaking races. Some scholars, promi-

nent among whom was the late Professor Bennett, of Cornell University, have maintained[1] that there was no ictus at all, its place being taken by what Bennett called "quantitative predominance." Of those who believe in the existence of an ictus—and they are at present in the majority—some think that both ictus and accent consisted of a very slight stress; that in verse the ictus took the place of the accent, which was disregarded in the reading of poetry. Others believe that, owing to the adoption of Greek literary models and Greek verse forms, educated Romans of the period from about 100 B.C. to 300 A.D. used the Greek musical or pitch accent. Since the ictus, if it existed, was unquestionably stress, this view also disposes of the "conflicts" between accent and ictus. It may be said in passing that the only strong argument which has been advanced against Professor Bennett's view is the statement, based upon experiments in the psychological laboratories, that rhythm without ictus is an impossibility. This statement Professor Bennett, in an unpublished paper to which I have had access,[2] questions on the ground that the experiments were made upon subjects of Teutonic race, to whom a stress accent was familiar and a pronunciation without stress was unnatural.

For many scholars the question of the nature of the classical Latin accent was settled by Professor Abbott's paper on "The Accent in Vulgar and Formal Latin,"[3] in which he maintained that while the accent of the common people continued to be one of stress, the educated Romans developed an accent in which pitch predominated. This view, which at first seems startling, if not improbable, is reasonable enough when we consider the extent to which Roman literature was based upon Greek, as well as the fact that to Romans of good education Greek was a second language, which was almost as familiar as the vernacular. Thus the emperor Claudius said to a foreigner who spoke both Greek and Latin: "cum utroque sermone nostro sis

[1] See Bennett, in *Amer. Jour. of Phil.*, XIX., pp. 361 ff. and XX., 412 ff. This view was expressed by Madvig, in his Latin Grammar of 1847, and it is supported by John Williams White in his work on The Verse of Greek Comedy, p. 9, and by others.

[2] "Theory and Practice in the Reading of Classical Verse." An abstract appears in *Univ. of Penna. Bulletin*, XX., 1 (Oct. 1, 1919), pp. 362 ff.

[3] *Classical Philology*, II., pp. 444 ff.

paratus."[4] The theory is also given strong support by the circumstance that both quantitative poetry and pitch accent by 400 A.D. were giving place to the native stress and to accentual poetry. For the detailed evidence I may refer to Abbott's paper and to one by Professor R. G. Kent in the *Transactions of the American Philological Association*, LI., pp. 19 ff.[5]

This theory of the nature and history of the Latin accent makes the metrical reading of Latin poetry, if not less difficult, at least more rational and, it is to be hoped, more uniform. It also makes possible the careful observance of the quantity of all long syllables, which would be difficult, and probably impossible, in such words as *ēvītābātur* and *dēsīderābātur*, if the accent were one of stress. Careful observance of quantity did not, of course, imply an abnormally slow delivery or a uniform length for every long syllable. Chœroboscus, a writer on Greek metrical theory, designates five degrees of length in syllables, and modern phoneticians recognize at least as many.

It is natural to infer, as certain linguistic phenomena indicate, that the common people as a whole did not adopt the fashionable pitch accent; but it was probably not without influence upon the speech of those who came most closely in contact with the upper classes or had social ambitions. Furthermore, the Greeks at Rome were not only the teachers of the children of the upper classes, but they filled many humbler positions and therefore were likely to influence all classes of society. Thus Juvenal writes:

> Quem vis hominem secum attulit ad nos;
> Grammaticus, rhetor, geometres, pictor, aliptes,
> Augur, schoenobates, medicus, magus; omnia novit
> Græculus esuriens. (*Sat.*, III., 75 ff.)

[4] Suet. *Claud.*, 42. 1; cf. Hor. *Odes*, III., 8, 5, docte sermones utriusque linguæ, and many similar utterances.

[5] See also Turner, *Classical Review*, 1912, pp. 147 ff., who finds no evidence in formal Latin from 250 B.C. to the end of the fourth century of our era in support of the opinion that the Latin accent was primarily one of stress; although, as Professor Abbott again points out in *Class. Phil.*, VIII., p. 92, vulgar Latin seems to furnish clear proof of the predominance of stress. Skutsch, in *Glotta*, IV., pp. 187 ff. suggests that the pre-literary first-syllable accent came from the Etruscans, and that the later three-syllable accent was due to Greek influence.

The new pitch accent became known to the commons also in the theatres, as they listened to the declamation of the comic and tragic actors. Cicero twice tells us[6] that all the whole audience cried out if a single syllable was pronounced too long or too short. That he referred only to the senators who sat with him in the orchestra, or at most added only the "fourteen rows" occupied by the knights, is made improbable by the context; for he prefaces the former statement with the remark, quotus quisque est qui teneat artem numerorum ac modorum, and follows the latter with the words, nec vero *multitudo* pedes novit aut ullos numeros tenet. And today auditors with good ears notice an unmetrical line in a Shakespearean play, or a false note in grand opera, even though they know nothing of musical theory or of meter.

In many instances, of course, the difference between long and short was obvious and significant. Thus Nero used to pun cruelly at the expense of his sainted predecessor, saying that Claudius had ceased inter homines morari, lengthening the first syllable of the last word, as Suetonius obligingly explains,[7] and thus changing its meaning from "linger" to "play the fool." We may compare hinc avium dulcedo ducit ad avium in the Auctor ad Herennium (iv, 29) and many similar word plays.[8]

As is well known, the Romans employed various devices for indicating the length of vowels. A doubled vowel—*e.g.*, PAASTOR, I, 551,[9] 132 B.C.; SEEDES, I, 1166, 104 B.C.—from about 134 B.C. until about 78 B.C., a period fixed by the testimony of inscriptions, as well as by the assignment of the device to the poet Accius,[10] who, if he was the first to employ it in Latin, borrowed it from the Oscan. For

[6] *Orat.*, 173 and *De Orat.*, III., 196.

[7] *Nero*, 33, 1. The tense of *iocabatur* indicates that Nero was proud of this witticism and used it more than once.

[8] It hardly seems necessary to say that the pronunciation of prose and poetry were the same. This is directly stated by Cicero, *Orator*, 190: 'Sit igitur hoc cognitum, in solutis etiam verbis inesse numeros eosdemque esse oratorios qui sunt poetici'; and by Quintilian, *Inst. Orat.* IX., 4, 61: 'Et in omni quidem corpore totoque, ut ita dixerim, tractu numerus insertus est; neque enim loqui possumus nisi syllabis brevibus ac longis, ex quibus pedes fiunt.'

[9] References like this are to the *Corpus Inscriptionum Latinarum*.

[10] Terentius Scaurus, *De Orthog.*, VII., 18, 12 K.

a long *i* Accius wrote *ei*, a usage which had come in before his time and continued through the seventh and eighth centuries of the city. The origin and history of this device, which was not very common in Latin, are discussed with a full list of examples by Bersu in Bezzenberger's Beiträge, xxiii, 252 ff.

In the time of Sulla a tall I came into use as the designation of the long vowel, without at once displacing *ei*; and at about the same time the apex was introduced to indicate the long quantity of the other vowels. These two devices continued in use until the latter part of the third century, the latest examples being found in v, 857, of the time of Diocletian, where we have seven apices in five words, three of which are over the diphthong *æ*. A much later instance, Nársés in XIV., 4059, of the year 565 A.D., represents a different use of the apex, to be mentioned later (p. 79).

The employment of these devices, their variation, and the results deduced from them are discussed at length by Bersu[11] and Christiansen.[12] One point, however, is fully treated neither by them nor, so far as I know, by anyone else: namely, the reason or reasons why some long vowels are marked and others are left unmarked. Almost the only contribution to this aspect of the phenomenon has been the very easy one of exploding the idea that the apex designated the word accent. In the light of the large number of apices on the final syllables of words, one can only wonder that that opinion was ever held by anyone. I have never been able to convince myself that these marks were put on at random, but I have thought that the examination of a large number of inscriptions would at least throw some light upon the question. This paper presents the beginning of such an investigation. In its entirety the problem is an enormous one, since it involves the consideration not only of all the indicated quantities, but also of many long vowels in inscriptions belonging to the period from 100 B.C. to 300 A.D. of which the quantities are not indicated. There is also the possibility, if not the probability, of a negative result; but there are certain to be by-products of some interest and value.

[11] L. c.

[12] *De Apicibus et I longis*, Husum, 1889.

The Roman grammarians are not wholly silent as to this point. Quintilian says:[13] "Longis syllabis omnibus adponere apicem ineptissimum est, quia plurimae natura ipsa verbi quod scribitur patent; sed interim necessarium, cum eadem littera alium atque alium intellectum prout correpta vel producta est facit: ut "malus" arborem significet an hominem non bonum apice distinguitur, "palus" aliud priore syllaba longa, aliud sequenti significat, et cum eadem littera nominativo casu brevis, ablativo longa est, utrum sequamur plerumque hac nota monendi sumus." In the time of Hadrian, Terentius Scaurus writes:[14] "Apices ibi poni debent ubi isdem litteris alia atque alia res designatur, ut 'venit' et 'vēnit,' 'aret' et 'āret,' 'legit' et 'lēgit,' ceteraque his similia. Super *i* tamen litteram apex non ponitur; melius enim I in longius producitur. Ceterae vocales, quia eodem ordine positae diversa significant, apice distinguuntur, ne legens dubitatione impediatur."

These statements are clear and definite. The second one made by Scaurus is confirmed by the inscriptions; for although there are twenty-two instances of an apex over *i* in the twelfth volume of the *C. I. L.* alone, that use is relatively very rare. An apex occasionally appears even over a tall I, as in XII., 890 and 3065 add. The other statement, in which Quintilian and Scaurus agree, points to a logical and helpful use of the apex, but unfortunately the statement is not confirmed by the inscriptions. The apex[15] is used in many instances where it does not serve to distinguish words or case-endings of the same spelling except for the quantity of the vowels, and frequently such words or case-endings are left unmarked. This fact, however, does not prevent us from accepting Quintilian's principle as one of those which regulate the use of the apex; for it will be seen that no principle of the kind is observed with uniformity. In fact, one is almost tempted to think at times that uniformity was deliberately avoided.

Of many thousand inscriptions which I have examined I have found only two in which all the long vowels are marked. As both

[13] *Inst. Orat.*, I., 7, 2-3.

[14] *De Orthog.*, VII., 33, 5 ff. K.

[15] For brevity, the term 'apex' is used here and in numerous other instances to include the apex and the I longa.

these inscriptions are short, probably no great significance is to be attached to their consistency, but they are interesting as rare specimens. One is VI., 30805, Sīlvánó Au. sacrum. C. Iúlius Castrénsis ex vótó (incidentally it may be remarked that the marking of a long vowel before *ns* is not very common). The other is XII., 2925, Iov*i*[16] O. M. Gallus Iúlius Honórátus V. S. L. M. There are numerous other inscriptions in which the number of marks is relatively large, such as III., 9960; V., 6786, 7430; VI., 4226; XII., 3219; XIV., 2553, quoted on p. 91, and many others. In some, on the contrary, only a few words are marked, generally, although not invariably, at the beginning: *e.g.*, VI., 30865, where the first and third lines, Pró salúte . . . Pontificis Maxim*i*, are accompanied by eleven lines containing ten long vowels, no one of which is marked; in VI., 10363, *inmúnis* is the only marked quantity in an inscription of eight lines.

Even a cursory examination shows that the apex and the I longa are not used in exactly the same way. Except for a few inevitable errors, the apex is much more consistently confined to the designation of long vowels, while the I longa has various other uses.[17] Furthermore, the I longa is decidedly more frequent than the apex. In those volumes of the *C. I. L.* which are provided with indices the examples of words marked with an apex are collected, but very few of the editors have essayed the enormous task of assembling all the tall I's. Some inscriptions have the I longa, but no instances of the apex, including the account of the Ludi Sæculares of 17 B.C. (VI., 32323); VI., 9992, for example, has twelve tall I's within a few lines, but not a single apex. In this connection it should be noted that there is another apex, frequently having the same form as the one which indicates a long vowel, which is used as a mark of punctuation, both between words and after abbreviations. This apex, which is attached to consonants and short vowels, as well as to long vowels, is commonly placed beside the letter instead of over it, as in VI., 838, ex' visu fecit' Evia' Helpis, where it serves to separate words, instead of the usual point. In VI., 31856, belonging to the early part of the reign of Marcus Aurelius, it serves to designate abbreviations: a rationib' próc trib'. Sometimes it stands over the letter, as in ḿ for

[16] An italicized *i* is used to designate the I longa.

[17] See Christiansen, l. c.

missus, IV., 1182, No. 6, and F́ for fecit, IX., 2082 B, a Christian inscription; cf. Auǵǵ. and lib., VI., 738, of the time of Septimius Severus; dec̆., XIV., 3023, and primiǵ., XIV., 2851. This apex is much less common than the one which designates a long vowel and is not likely to be mistaken for it. It may have led to the use of the long vowel apex as a mark of punctuation after abbreviations and at the end of sentences and clauses (see p. 86), when such a use is consistent with its employment as a designation of long quantity; as in féc. for fecerunt, VI., 4027, and elsewhere. Conversely, the long vowel apex may have influenced the form of the other apex, which perhaps came from the commoner mark of punctuation /.

It would seem that the best results would be obtained by examining some of the longer official inscriptions of the best period, and we may begin with the Monumentum Ancyranum, the great inscription in which the Emperor Augustus recorded the events of his reign.[18] In examining this document we must, of course, bear in mind that it is a copy of the lost original in Rome. We shall probably be safe, however, in assuming that the stone-cutter followed his copy as carefully in adding such marks as he did in other respects, and this assumption receives support from the fact that in the beginning and end of the inscription, which were not copied from the Roman original, errors are more frequent than in the body of the record, where they are very rare.

The inscription proper contains approximately 1,884 words and 1,399 long vowel-quantities. Of the latter 487, or about 34 per cent., are indicated by apices or by the tall I. The marks are for the most part limited to one on each word, but forty words have two marks and two have three marks each.[19] The modern scholar, in spite of the silence of the native grammarians, might be led to inquire whether the so-called "hidden quantities" are designated by marks. We find

[18] Suet. *Aug.*, 101, 4; Cassius Dio, 56, 33.

[19] The counting has been done with care, and while it would be rash to claim absolute accuracy, the percentage of error is certainly not large enough to affect any of the conclusions. The writer has made complete word indices of the Monumentum Ancyranum (designated as M. A.) and the Speech of Claudius (S. C.), and full lists of all the examples of the different positions and uses of the apex. Considerations of space prevent the printing of these lists, but they form a reasonable guarantee of accuracy.

that in the M. A. twenty-six such vowels are marked, but they amount to only about 20 per cent. of the 138 hidden quantities in the whole inscription. The only repetitions are lústrum and lústro, which occur three times each. A vowel before *ns* is marked four times, but is left unmarked sixty-nine times. These results in themselves are almost enough to show that "hidden quantities" are no more frequently marked than others. In fact, in nine words in which hidden quantities are not marked we find other long vowels marked; thus *consulátú* occurs four times.

Although, of course, the apex did not designate the accent, it naturally occurs frequently on accented syllables, since an accented penult often contains a long vowel, which is sometimes the only long vowel of the word. In the M. A. 214 accented syllables are marked, while there are 208 accented syllables containing long vowels which are not marked; the count is confined to words which have apices, and monosyllables are not included.

Quintilian's rule is observed in seventy-eight cases, while in forty instances marks are omitted which would differentiate words or forms. These figures, however, are the result of giving the rule the most liberal interpretation possible, including, for example, all cases of *īs* and not merely those from words which also have forms in *-ĭs*, and adverbs like *antea* as well as ablatives in *-a*. If we confine the count to forms which could actually be mistaken for others, we have thirty-nine marked vowels and twenty-three unmarked.

Considering next the syllables on which the marks are found, there are two instances of marks on the sixth syllable from the end, which in each case is the first syllable of the word: frúmentátiónes and próvincialibus. The former is one of two words in the entire inscription which have three marks. The latter has only one mark, although it contains another long vowel.

Eight words have marks on the fifth syllable from the end, including ínsaliare and ínconsulatu, in which the preposition is treated as a part of the word. The mark on ínconsulatu is one of the very few errors in the main body of the inscription. Only two words have additional marks, úniversórum and múnicipiis; in one other only the

first syllable of the word is preserved, so that it is impossible to say whether or not there was a mark on the only other long vowel.

Fourteen words have marks on the fourth syllable from the end. In eight instances this is also the first syllable of the word, and in eight instances all the surviving long vowels are marked. Four of the words have other marks as well, and there is one repetition, úniversus and úniver(si). It is perhaps significant that in several of the words, amícitia, Óceanus, tribúnicia, úniversus, for example, we have a single long vowel in the neighborhood of short sounds, where the quantity is especially important for correct pronunciation.

On the last three syllables marks are much more frequent, which is not surprising when we consider the relative number of long and short words in the language.[20] On the antepenult we have sixty-one marks. In thirty-four of these the antepenult is the first syllable of the word and in forty-eight it is the accented syllable. In five of the twelve instances in which the antepenult is not the accented syllable the accented penult is also marked, and in four others the accented penult contains a short vowel followed by two consonants, leaving only three words in which the accented syllable is not marked when it could be marked. There are several repetitions: cúriam twice; óstium twice;[21] frúmentum twice; aerárium twice. Eight hidden quantities are marked and only one is unmarked

For the penult the figures greatly increase, there being 167 examples of an indication of the length of that syllable. In every case but one the penult is the accented syllable. The solitary exception is civica, one of the few errors in the body of the inscription, a short i being written with I longa. In fifty-four instances the penult is the first syllable of the word.

The greatest number of marks, 204, is found on the final syllable. In twenty-nine words there is also a second mark, usually on the penult. Thirty-one monosyllables have not been included.

It does not seem probable that any particular long vowel was more

[20] In the M. A. there are but four words of six syllables, twenty-four of five syllables, and ninety-four of four syllables.

[21] For brevity, mere differences in case, such as ostium, ostio, are not noted separately.

likely to be marked than another. The figures for the use of the apex in the M. A. are given by Christiansen[22] as follows:

a	o	e	u	i	ae
157	102	65	49	1	1

If we subtract the total of these, 365, from the total number of marks, 487, we get 112 as the number of tall I's, giving that letter second place. In IX., 3060, all the apices, four in number, are on the letter *a*, but no stress is to be laid upon this, and there seems to be no reason for marking one long vowel more frequently than another; in VI., 6191, all the apices are on final *o*.

In the M. A., and the same thing seems to be true of some other inscriptions, the marks show a decided tendency to flock together. Sometimes entire sections have few indications of quantity (*e.g.*, I., 4, with the exception of the first two lines, I., 5; I., 13, the latter part of II., 8; IV., 22, and IV., 30); others, on the contrary, have many marks, such as III., 14; IV., 23, etc. It is very common for two successive words in agreement to have marks, whether the vowels are the same, as in aurí coronárí, triumphós meós, eá pecuniá; or different, as in reditú meó, octingentós pedés, curulís triumphós. In some instances three successive words are marked, as summá sacrá viá, or we find two successive words with two marks each, as cúriá Iúliá, Díví Iúlí. An example of striking inconsistency appears in V., 28, in the phrase "in Africa, Sicilia, Macedoniá utraque Hispániá . . . Asia, Syria, Galliá Narbonensi, Pisidia," where it is difficult to see why three of the ablatives should be marked and the others not.

We may also note a tendency to mark series of words which are not in agreement, such as impensá grandi reféci and rivos aquarum complúribus locis vetustáte labentés reféci (both in IV., 20). Since the endings -orum and -arum are often marked, the absence of a mark on aquarum is also noteworthy. In III., 16, we have a very long series: pecuniam (pro) agris, quós in consulátú meó quárto et posteá consulibus M. C(rasso e)t Cn. Lentulo augure absignávi militibus, solví múnicipis. At first thought the eleven marks in two lines and a half seem no more remarkable than their omission in the words consulibus . . . augure, but as a matter of fact the latter phrase, although of some length, contains only two long vowels. Moreover,

[22] L. c., p. 13.

as we have seen, a long vowel before *ns* is seldom marked in the M. A., and the same thing is true of final *o*. The absence of a mark on militibus is perhaps more noteworthy. For some reason or other that word, in spite of frequent occurrences, is never marked, although militaria occurs. It is possibly not too fanciful, in the light of the honorary use of the apex mentioned below, to connect this omission with Augustus's attitude towards the soldiers.[23] In general, the marking of the long vowels seems to be to a certain extent a habit, which once begun is carried on for a time, dropped and resumed, a view which receives confirmation from the usage in other inscriptions.

The apex seems to be used, as the I longa undoubtedly was, to add dignity or majesty, or to emphasize certain words with that end in view. D*í*v*í* occurs twice in the M. A. and is frequent throughout the period from Augustus to Commodus. Iúlius and Iúlia occur seven times with one or two marks (the latter is possible only in the oblique cases), and the latter once without a mark. We find tribúnicia and tribuniciá of the emperor's tribunician power beside tribu(nic)iæ and an indecisive (trib)uniciæ. Consulés is found only twice out of twenty-nine occurrences of the word, but we have consulátum once, and consulátú four times, of the consulates of Augustus. It may be fanciful to connect three occurrences of Capitolium without marks with Augustus's minimizing of the importance of Jupiter and the Capitol as compared with Apollo and the Palatine, but Capitólió occurs in VI., 2027, B (perhaps of the year 37 A.D.) and 2080, 9 (120 A.D.), and Capitolió in VI., 2042, 6 and 59, 2059, 41, all inscriptions of the Arval Brethren. If we add the frequent occurrences of D*í*s Mánibus, we may seem to be justified in the conclusion that this feature plays some part, although it did not extend, as did the use of the tall I, to short vowels. It is rather striking in this connection that in the inscriptions dedicated by lictors we find lictor in VI., 1871, 1881, 1892 and 1905; in the inscriptions of viatores, viátóri, VI., 1921; cf. 1932, a. Other official titles which occur with

[23] Suet. *Aug.*, 25, 1, neque post bella civilia aut in contione aut per edictum ullos militum commilitones appellabat, sed milites, ac ne a filiis quidem aut privignis suis imperio præditis aliter appellari passus est, ambitiosius id existimans quam aut ratio militaris aut temporum quies aut sua domusque suæ maiestas postularet. For the different attitude of Julius Cæsar, see Suet, *Jul.*, 65 ff.

marks are Augustales, accensus, curator, flamen, patronus, procurator, legatus (with two marks, V., 4359), rex (five times in the M. A., twice régés); in próconsul (III., 9960, and elsewhere) and prónepos (IX., 3176; III., 14147, 1) the mark may indicate the prefix (see below). Marks are not very common in the Pompeian inscriptions, but we find vénatió in IV., 3884, venátio in 1186, and venatió in 1190, doubtless for emphasis; cf. véla, IV., 1190. The same general feature appears in the designation of military and other prizes, as in corónà áureá, V., 7003.

The following words which occur more than once are always marked: curia three times, flumen twice, lustrum six times, manibiis or manibis four times, ostium twice, Penates twice, reciperavi twice, refeci three times, solvi twice with solutis, frumentum twice with frumentationes. The following, on the other hand, show a variation in usage: auxi and auxi, accepérunt and acceperunt, cives three times but cives once, denáriós twice but denarium, and others.

In a number of instances the mark coincides with a word division in compounds as in undéviginti, quinquáginta, rés publica, rei publicae; or indicates a suffix, as in aerárium, anniversárium, etc. There seems to be no other good reason for the frequent marking of nómen (three times in the M. A., II., 5513; V., 7430; VI., 2042, a, 26; 2059, 38), testámentum (III., 10867; V., 969; XII., 1375, 3593); cf. órnámentís, XII., 3203, and ornámentís, XII., 3219. Prefixes are perhaps indicated in the same way, as in reféci, dedúxi and in aéde, the only example of an apex over *ae* in the M. A., although that usage becomes common later. In the marking of case-endings, which is frequent, we should expect Quintilian's rule to be followed, but the expectation is not fulfilled. We find the endings *-arum* and *-orum* marked, *-es* in the nominative and accusative plural, *-is* in the dative-ablative, and *-os* in the accusative, plural, no one of which is likely to be mistaken for another form, as well as the ablative in *a*, the accusative plural in *-īs*, and the forms in *-ūs* of the u-declension, where the marks differentiate the cases from the nominative, the genitive singular, and the forms in -ūs. In the M. A. -o of the dative-ablative singular is rarely, if ever, marked, but elsewhere it is frequently given an apex; seventy-four times in *C. I. L.*, XII. The forms of the a-declension in *-ae* are not marked in the M. A., but have the apex

forty-seven times in *C. I. L.*, XIV., twenty-eight times in XII. and eighteen times in V.

There is a decided tendency, for which there seems to be no obvious reason, to mark the penultimate or ultima, or both, in perfect tenses: vici, féci, cépi; auxí, fuí, iuví, feci; égi, reféci, misi. To this may be added over twenty examples of fécit, fécerunt, fécerit, etc. (seven of fécit in the index to *C. I. L.*, XIV.), nuncupávit, VI., 2042 d, 25; cooptavérunt, VI., 2078, 39; vénerit, VI., 1932 a; decrévit, VI., 894, and many others.

The use of an apex to indicate punctuation has already been spoken of.[24] There are in the M. A. a good many instances of a mark on a final long vowel preceding a comma, a period, or a section mark in the original; in some of the places with section marks no punctuation would be used in English. Noteworthy in this connection is the phrase in quo triginta róstratae náves triremes a(ut birem)és, § (IV., 23), where the change from a penultimate accent to one on the last syllable of biremes, whether accompanied by another mark on the penult or not, is most easily explained as indicating a comma. Also noteworthy is the sentence rívos . . . reféci (IV., 20), quoted on p. 83, which is followed by a comma and a section mark.

We may now consider the speech of the emperor Claudius at Lugdunum, of the year 48 A.D. (XIII., 1668). The total number of long vowels is 498, of which 130 have indications of quantity, a percentage somewhat smaller than in the M. A. There are but six words with two marks and none with three. Of forty-five "hidden quantities" eleven are marked, a relatively larger number than in the M. A., but hardly enough to indicate a decided tendency. There are forty-nine instances of I longa, and the apices are distributed as follows: a, 32; o, 20; e, 13; u, 12. This does not differ greatly from the usage of the M. A., except that the I longa is relatively more frequent. There seem to be no short vowels which are marked with the apex or the tall I, and Quintilian's rule is observed with greater frequency than in the earlier inscription.

The same tendency to mark vowels in successive words is to be observed in the S. C. as in the M. A. Combinations of two words

[24] P. 80.

are especially common, although the marking of the same case-endings in successive words is rare. We find hác cívitate, statúsque rés p(ublica), Sabinís veniéns. Combinations of three words are less frequent: Ancó Márcio, Príscus; hóc ipsó consulári; secúram átergo pácem. We have one group of four words: superbi morés invísi cívitati, but longer ones do not seem to occur. There are fewer indications of a use of the marks for dignity or emphasis than in the M. A. Here perhaps belong cívitas, which occurs four times, and cívili, Dívus twice, rés publica four times. We have a single long vowel marked in the neighborhood of short vowels, where the proper observance of the quantity is especially important, in Óceanus (also marked in the M. A.), útilitate, ornátissima. The most frequent use of the marks seems to be in the indication of case-endings, prefixes and suffixes: coloniárum, bonórum, átergo, rés publica, approbáre, tenuére, trádere, translátum, díligo, díducta, exáctus, invísi. It must be remembered, however, that in several of these words the vowel which is marked is the only long vowel; also, in all these categories, that there may be other reasons for the marking of a vowel than those which determine the category. The marking of tenuére seems to be in accordance with Quintilian's rule, but in approbáre it is unnecessary.

A few words are marked in all their occurrences: civitas, Divus, casus (twice with two marks), fines; perhaps less stress is to be laid upon dicere and dixi, translatum and translata, venisti and veniens. The use of the marks to indicate punctuation is much less frequent than in the M. A.

It may be convenient to arrange the distribution of the marks in the two inscriptions in tabular form:

	Total Long Vowels.	Total Number of Marks.	Words with		Syllables Marked Counting from the End of the Word.						Monosyllables.	Hidden Quantities.		Accented Syllables.		Quintilian's Rule.	
			Two Marks.	Three Marks.	1	2	3	4	5	6		Marked	Not Marked.	Marked	Not Marked.	Observed.	Not Observed.
M.A.	1,309	487	40	2	204	167	61	14	8	2	31	26	112	214	208	78	40
S.C.	498	130	6	0	50	42	15	6	1	0	14	11	34	52	55	19	4

We may now pass to a general examination of the inscriptions, which shows that the usage was most common in Rome and Italy, in Cisalpine Gaul and Gallia Narbonensis, and in Spain. It was more frequent in central than in southern Italy. The inscriptions of Africa show very few examples, those of Britain none. The use of such marks is most frequent and accurate in official inscriptions and those of the educated. With the common people, so far as they use the marks at all, the usage seems to be imitative and sporadic. This view is apparently confirmed by the use of marks in certain formulas, such as "libertis libertabusque posterisque eorum," where it is common to find some or (rarely) all of the six long quantities indicated, especially by the I longa.[25]

In conclusion, a few additions to, or confirmations of, usages found in the M. A. and S. C. may be given. As has already been noted in connection with case-endings, an apex on the diphthong *æ* (usually on the first vowel, less commonly on the second) is of frequent occurrence. This also appears in syllables which are not case-endings. Christiansen[26] gives thirty-two instances, of which twenty-four occur in the Acta Fratrum Arvalium, and very many examples may be added to his list. His remark, p. 17, that an apex is rare over *æ* and *au* is misleading; for the occurrence of an apex over any diphthong other than *æ* is exceedingly rare, while over *æ* it is fairly common. His suggestion that the purpose of the mark was to distinguish *æ* representing *ē* from the same diphthong representing *ĕ* seems doubtful; for although we find *æques* for *eques* in VI., 3409, of 197 A.D., the general use of *æ* for short *e* was certainly not early enough to account for the apex over *æ*.

The greatest number of indicated quantities in any one word of the M. A. and the S. C. is three, and the two inscriptions together contain only two such words. In VI., 11466, we find *ínfélícissimi* with four marks, including the somewhat rare designation of a long vowel before *nf*. Three marks occur several times, *e.g.*: Lúsitána twice, II., 5390; dédicátióne, III., 10767; quaéstórió, III., 11654; Fortúnátó, V., 1066; Narbónésés, XII., 4393; vénáliciário, V., 3349;

[25] VI., 9992, 10170, 10401, 10673, 11537; cf. 1805, 1917, 1921.

[26] L. c., p. 13.

dónátó, VI., 1377, 8; prócédéns, VI., 1527; pácátó, VI., 1527; viátóri, VI., 1921, 1935 a; félici, VI., 12133; honóràtó, XII., 3219; Honóráto, XII., 3637; tógátórum, XIV., 409, 14; fátálés, XIV., 2553; adórátúros, XIV., 3608, 17; órátióne, XIV., 3608, 31.

There is a decided tendency to mark quantities in personal names, perhaps as an indication of honor (see p. 84), or in some cases to insure their correct pronunciation: many men object to having their names misspelled or mispronounced. The latter would seem to be the reason for Caninio, M. A., III., 16, in a list of ten consuls, of which no other name has (or apparently needs) an indication of quantity: Cáninius occurs in X., 3036, Cáninió twice in XIV., 2556, and in VI., 14343 Caninio is the only word marked in an inscription of five lines. Names which are frequently marked are Iulius and Iulia (see p. 84), of which I have thirty-one examples, by no means a complete collection; Marcus and its derivatives, V., 555, 7678, XIV., 2802, etc.; Cornelius, III., 8786, 11690, V., 757 add., 909 add., 1179, twice, six times in *C. I. L.*, XII., etc.; Mars and its derivatives, Antonius and derivatives, V., 115 twice, seven examples in III.; Pollio, V., 5906, VI., 1829; four times in XII.

Nouns indicating relationship are frequently marked, perhaps as a token of honor or respect: for example, uxori, II., 2642, III., 8786 twice, VI., 1859, 1975, and fourteen other examples; filius, VI., 880, 1825 and elsewhere, four times in the M. A., but unmarked eight times; frater, XIV., 2637, 3608, 19 and frequently; mater, V., 1179, 6013, 6091, 7678 and frequently. In the last-named inscription fratri, matri, uxori and sorori are all marked, although a second occurrence of sorori is unmarked. In V., 1179, matri is one of only two words marked in an inscription of six lines, the other mark being over *ae*. It is doubtless to this tendency that the erroneous marking of cóniugi is due in V., 1066, and VI., 9914.

The preposition a is often marked: six times in the M. A. (twice unmarked) and four times in the S. C. (once unmarked); also II., 3426, III., 12046, VI., 4312, 9970, XIV., 254, 409, 14; 3543, and in numerous other instances. It seems probable that the mark serves merely to separate the preposition from a following noun, whether the two are written as one word or separately, and we also find *e*, *de* and *pro* marked. The marking of monosyllabic words, however, is

rather frequent (thirty-one examples in the M. A. and fourteen in the S. C.).

In metrical inscriptions the long marks frequently coincide with the ictus (or whatever term we may prefer to use), but here, too, there is no uniformity. All the apices seldom coincide with the ictus,[27] nor is every ictus marked in any inscription. A typical instance of irregularity occurs in IX., 60, where we have eleven hexameter lines, of which two have no marks, five have one mark, one has two marks, one three, and two four. The lines with four marks, which are not consecutive lines, read as follows:

> Hic meas déposui cúras omnésque labores.
> Fortuna infráctá ter me fessum recreásti.

In the former line three of the four apices coincide with the ictus. The one over curas does not, but the proper length of the u is important, perhaps especially important, for the rhythm of the verse. We find a corresponding syllable marked elsewhere; for example, in the following lines:

> Víva viro placuí prima et cátissimum coniunx. VI., 6593.
> Quós pius saepe colit fráter coniunxque puellae. VI., 28877.

Since in the last example the word in question is frater, there is room for doubt, as often happens, as to the reason for the mark. The disregard of *s* for making position in the first and fourth of the above lines and the grammatical error in the third point to a vulgar origin. It is noteworthy that in the second line two of the marks, being over hidden quantities, are superfluous so far as the meter is concerned, although they are important for the correct pronunciation.

In the inscription IX., 60, twelve apices, in eleven lines, coincide with the ictus, while seven do not. With this inscription we may contrast VI., 9797, of nineteen lines, in which the apices and tall I's are very numerous, although in no line do the marks coincide throughout with the ictus. In another inscription, XIV., 2553, consisting of two elegiac couplets, nearly all the long quantities are marked, but three long vowels on which an ictus falls are not marked. The in-

[27] There is one example in a pentameter (VI., 6593), post obitus satis hác fémina láude nitet.

scription reads as follows:

Ólla Ī Secundáe
Fátálés moneó né quis mé lúgeat orbi
Namque Secunda fui nunc tegor é cinere.
Hic ego secúris iaceo super omnibus úna
Nátális quia nós septimus ussit amor.
Nátális monumenti III Idús Maiás.

It may seem that the inferences which have been drawn are based upon a small number of examples. That is quite true, but it must be remembered that the total number of marked words is comparatively small, and that among these the repetitions are far from numerous. In the index to *C. I. L.*, XIV., for example, there are 211 words with apices; among these there are but 33 repetitions and only fifteen instances in which words are repeated three times or more. The occurrence, therefore, of Mánibus eight times, of fécit seven times, and of Iúlius, Iúlia five times may fairly be regarded as significant, especially when the number of examples of these forms is increased from other volumes. The study has suggested to the writer a number of lines of investigation, which he hopes to follow out at some future time.

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SOME PECULIARITIES OF THE NOVÆ.

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(*Read April 21, 1922.*)

There are no more interesting objects in the sky than the novæ. The suddenness with which they rise from a faint or obscure condition or even from absolute invisibility, sometimes to outrank all the other stars in the heavens, in some cases increasing their light as much as a hundred thousand fold, is very wonderful. This great rise in brightness requires only a few hours or a few days at most. Their rapid physical changes, the various colors through which they pass in their declining light, their later change to apparently a nebulous state, and their final return to what seems to be their original condition which they attain in a few years' time, make them of the greatest interest from every point of view.

The novæ remain at their greatest brightness for a very brief period, which in some cases can be counted in hours. They then begin to fade, at first rather rapidly, then slowly. With many halts and minor outbursts they finally, in a few years' time, say from eight to ten or fifteen years, return to their original brightness. This interval of decline seems to vary in different novæ. Though they thus follow the same process of rapid change in brightness and physical condition, there are decided differences and peculiarities among them that might suggest great dissimilarity in their pre-nova state.

One fact that is peculiar to them, though not necessarily definite in its character, is that with perhaps one exception (that of Nova Coronæ of 1866) all of the novæ are found in the Milky Way. Of course, there are vastly more stars in the Milky Way than outside of it and therefore a greater chance for a nova to appear in it. There are other evidences, however, that they really belong there.

What was the original condition of a nova? Little is known of the early history of these bodies—before their outburst of light. We

know something of the visual history of Nova Coronæ Borealis of 1866, for it was observed at Bonn, previous to its outburst, as BD $+ 26° 2765$ of the 9.5 magnitude. Though this star appeared some distance from the Milky Way (in which the novæ apparently belong), there is no other reason to question its character as a nova.

There is now some evidence that possibly the novæ were all variable stars in the earlier stages of their history, but the proof is not conclusive. Photography has shown that at least two of these stars were small variables before they became novæ. Nova Persei is the best example. Thanks to the splendid collection of stellar photographs of the Harvard College Observatory extending over more than a third of a century, we know that previous to its sudden appearance in 1901, when it became brighter than the first magnitude, Nova Persei was a small variable star of about 14th magnitude. After its outburst in 1901 it slowly faded to its original brightness and again became a small variable star—varying perhaps just as it did before the blaze-up. Apparently the great outburst was only an interruption in its regular variability, which is now past and forgotten.

The other, Nova Aquilæ of 1918, which became brighter than any star in the sky with the exception of Sirius and perhaps Canopus, previous to June 8 was also a small variable star of 10.5 magnitude. It has not yet sunk to its original faintness, certainly not from a photographic standpoint, though it has nearly done so. It is only by comparing the present photographs of it with the early ones that we can tell when this event occurs. It will probably resume its variability later on. Of course, these statements of variability have nothing to do with the peculiar fluctuations of the light of a nova in its immediate decline from its maximum.

As we have said, it would appear that the outburst in these two stars was only an interruption in their variability. Professor Turner has made an attempt to connect up the present variability of Nova Persei with that before the great change occurred, but he has not yet obtained a satisfactory result.

Perhaps the phenomena of Nova Aquilæ were of even greater interest than those of Nova Persei. This was probably due to the extremely great brilliancy of the star at its maximum. Its entire

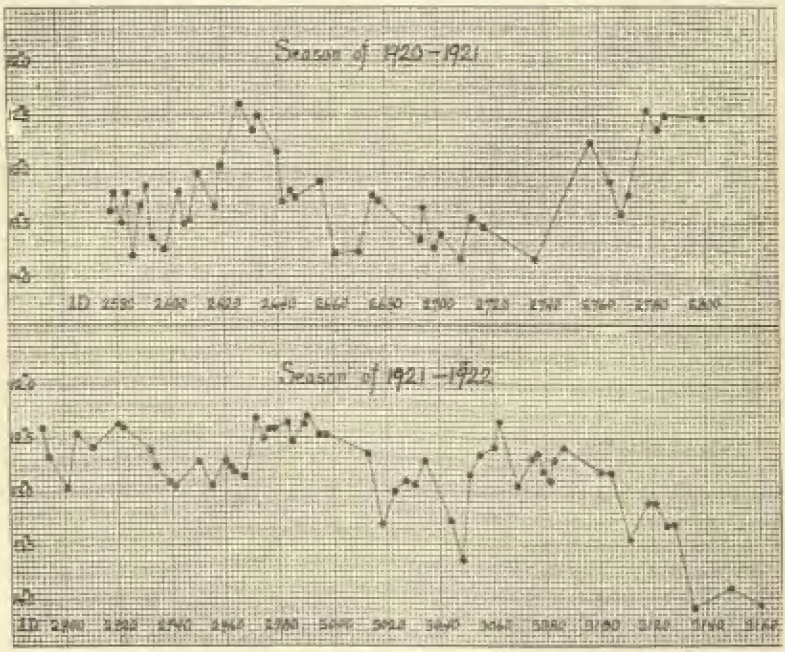
declining phase was brighter than that of any other modern nova, for it was originally a brighter star than any of them with the exception of Nova Coronæ of 1866, whose range of brightness was not nearly so great. Before the outburst Nova Aquilæ was a comparatively bright star of 10.5 magnitude. Nearly all of the other novæ have started either from a very faint condition or from one of entire invisibility. It is easy to show that if some of the other novæ had been as bright as Nova Aquilæ in the beginning they would have much outranked it when at their maximum brightness. This was specially so in the case of Nova Persei, which would have been a brighter star than Nova Aquilæ, because its entire increase of brightness was much the greater of the two. This has given some hope that the great nova of 1572 (Tycho's star), which was by far the brightest nova on record, may have been a considerable star when the outburst occurred, and that it may now be visible as such. But we have no means of identifying it. The uncertainty of Tycho's position of it and the rich region of the Milky Way in which it exists makes it quite impossible to identify it among the many stars in its immediate region. Should, however, a small variable star be found sufficiently near to its assumed place, it would add much to the supposition that the great nova had been found. It would be interesting to recover this star now, for it might be bright enough for spectroscopic study. The great lapse of time since it appeared in 1572 might give results of much value in the history of these stars.

There seems to be good claim that P Cygni of 1600 was a real nova. It is therefore the oldest nova that we can certainly identify. If we follow the information we already have of the novæ, it would seem that this star, if a true nova, was probably visible to the naked eye previous to its outburst.

The next oldest, and one where there is no doubt of identity, is Hind's Nova Ophiuchi of 1848. Though this star was not bright enough to attract very wide attention, a few astronomers observed it carefully. Bond, at the Harvard College Observatory, measured its position with respect to a small star near it. From this it is identified with certainty. While bright enough, it was also observed at Bonn and is BD — 12° 4633. Recent observations show that the star is probably variable.

Nova Cygni of 1876, which was discovered by Schmidt at Athens, attained the second magnitude and was therefore a very bright object to the naked eye. It has long since returned to what was probably its original condition—that of a very faint star of perhaps 15th magnitude. For many years at least it has been irregularly variable with a light range of over one magnitude.

The old novæ that are now variable do not seem to have any definite periods. They are very irregular in their fluctuations and



Light changes of Nova Persei.

in the extent of their light changes. These light changes do not resemble those of the ordinary variable star, for their variability seems to be of a spasmodic nature with no distinct period. It is therefore very important to know if their original variability was of this same erratic nature, so that we can tell if they have entirely resumed their original condition. Photography will play a highly important part in answering this question for some of the future novæ. At present, for Nova Persei and Nova Aquilæ, there is not enough material to tell us what their pre-nova state really was, fur-

ther than that they were variable. The erratic variation of the light of Nova Persei, for two consecutive seasons, is shown in the accompanying diagrams. It will be seen that the extreme variation of the star is about two magnitudes. The two lower sets of figures (J. D.) are the Julian Day; those on the side are magnitude. These are on Seares's scale.

It must be borne in mind, however, that not all the novæ become variable stars on regaining their normal condition. Nova Lacertæ of 1910 (Espin), up to the most recent observations after its return to its original brightness, is perfectly steady in its light, or if there is any fluctuation it is too small to be detected by ordinary means.

One of the first of the novæ to be carefully studied with the spectroscope was Nova Aurigæ of 1891. This star was discovered visually by Anderson in January of 1892. It was later found that the Harvard College Observatory photographs showed it as early as December 10, 1891.* Though it never became very bright, it gave more information of the physical condition of these bodies than any previous nova, mainly from the fact that the spectroscope, then but fairly starting on its wonderful career, aided by photography, was applied to the study of its light. We know nothing of the history of this star before its appearance in 1891.* It has probably long ago returned to its original brightness. There are some suggestions in recent observations of it that it is now also variable.

In its early stage, when in the decline from its maximum, this star showed one remarkable feature that has not been repeated in any other nova. In April of 1892 Mr. Burnham followed it with the 36-inch telescope of the Lick Observatory until it got too near the sun for observation. When he last observed it, on April 26, it had faded to 16th magnitude. Having passed the sun, it was found by Campbell in August of the same year to have risen to 9½ magnitude, and was a conspicuous telescopic object. It had apparently increased its brightness by six or seven magnitudes. In again fading it does not seem to have reached the low magnitude at which Mr. Burnham had seen it in the last of April, 1892. It is now about 14th or 15th

* A photograph taken by Dr. Max Wolf at Heidelberg, Germany, on December 8, 1891, shows nothing in the place of Nova Aurigæ as bright as the eighth magnitude. The star, therefore, must have made its appearance between December 8 and December 10, 1891.

magnitude. As I have stated, no other nova is known to have shown this second bright maximum.

Some of the novæ present very beautiful phenomena, especially in the wonderful colors they sometimes exhibit in their declining phases. At first these stars seem to be white; very soon they become red and pass through various colors, finally ending in a more or less colorless condition, their faintness preventing any appearance of color in them, if it existed. Their most beautiful colors, however, are seen in the telescope, usually after they have ceased to be visible to the naked eye—if they have been so bright as that. I would speak specially of two of these stars of recent years which have shown the most beautiful colors.

In March of 1919 Nova Aquilæ, in the telescope, was the most exquisite and intense blue, a color that no other star in the heavens could match. In September, 1920, Denning's Nova Cygni was a most remarkable object in a large telescope. At the ordinary focus was a white or nearly white stellar image surrounded by a brilliant system of crimson rays. At 9 mm. outside the focus the image was very small and beautifully scarlet. It was surrounded by splendid blue and golden rays. In one position of focus there was no central image, but a system of rays radiating from the center. For the first half of their length these rays were golden, while their outer half was crimson. The crimson color was due to the α Hydrogen line which was then specially strong in the spectrum of the star and gave a beautiful scarlet stellar image 9 mm. outside the normal focus.

The two great novæ of recent years, Nova Persei and Nova Aquilæ, have shown in their declining phase a remarkable phenomenon—that of a measurable planetary disc instead of the stellar point which all stars exhibit. In the case of Nova Aquilæ this measurable disc lasted for at least two months and seemed to be gradually increasing in size. On October 5, 1918, its measured diameter was $0''.8$, while on December 14 it was $1''.8$. The disc was sharply defined and its light was dull and planetary. Later the star resumed the regular stellar image. Aitken, in August and September of 1919, found, with the 36-inch telescope of the Lick Observatory, that with the telescope set for the focus of the nebulium lines N_1 and N_2, 8 mm. outside the normal focus, there was a definite disc $2''.4$ in diameter.

This was verified spectroscopically by Moore and was increasing in size.

In a previous paper (*Monthly Notices of the Royal Astronomical Society*, Vol. 72, p. 673, June, 1912) I have criticized some of the theories concerning the novæ; the encounter with a nebula, the collision theory, etc. In this connection was also criticized the theory that the apparent recession of nebulous matter from Nova Persei was due to the successive illumination of the details of a nebula in which the star was placed, by the outgoing light from the nova. This did not seem satisfactory because it was shown that certain details, such as the "arrow head" in the photographs by Ritchey and Perrine, were in actual motion away from the star and could not be due to light reflection from the nebula. This light reflection theory still seems to hold with some astronomers. There is much question, however, as to its correctness. Recent photographs of Nova Persei have been made by Lampland at the Lowell Observatory, not of the original nebulosities, for they are gone, but of the new nebulosity of 1916, December 16, which, as shown by the photographs by Professor Lampland, is moving out very slowly from the star. This slow motion, if its slowness is not due to motion nearly in the line of sight, seems to have no relation to the rapid movement of the masses in the earlier photographs of 1901, and it certainly can not be identified with any of the details of those pictures. Nor can the present nebulous disc about the star as photographed at Mount Wilson—so like a planetary nebula—be traced on the earlier photographs.

The most satisfactory theory to account for the novæ, especially with respect to Nova Persei, is that the forces of equilibrium of the interior of the star were disturbed, and that there was an outburst in the nature of an explosion, which, though it produced the great brilliancy in 1901, apparently neither destroyed the star nor very seriously affected its variability.

Summary.

The novæ usually appear suddenly in the sky. Their advent is entirely unheralded and it is only in recent years, through photography, that anything has been known of their former existence. The reason for their sudden appearance is unknown. It is probable that

for some cause the forces of equilibrium in the star become unstable and there occurs a great outburst of light in the nature of an explosion. This does not actually destroy the star, for later on, in the course of a few years, it returns to its original brightness and probably to its former physical condition. This is shown by the fact that Nova Persei of 1901 was originally a small variable star. Having now returned to its original brightness, it is again variable.

Sometimes in their decline from maximum these stars present a well-defined, measurable, planetary disc and finally become perfectly stellar again. Always, at one period of their decline, they present a beautiful, sharply defined, crimson image 9 mm. outside the normal focus in such a telescope as the 40-inch refractor of the Yerkes Observatory. This is due to the presence of the α Hydrogen line, which is then strong in their spectra. This always occurs several weeks after their first appearance.

When they first appear they are very white. They soon turn red and in the end become white or colorless again. In their decline they pass through many very beautiful colors, such as an intense blue and gold and crimson—colors that no ordinary star shows in such exquisite purity. This is all due to the nature of their light, through their changing physical condition.

YERKES OBSERVATORY,
WILLIAMS BAY, WISCONSIN,
April 15, 1922.

THE RELATIONS OF THE RETINAL IMAGE TO ANIMAL REACTIONS.

By G. H. PARKER.

(*Read April 21, 1922.*)

During the past generation the interpretation of animal activities has undergone a profound change. To the earlier naturalists even the simplest animals were supposed to be endowed with sensations, preferences, desires, volitions, and the like, which, though simplified in form, were nevertheless the same as those in ourselves. But this so-called anthropomorphic viewpoint was soon found to present many difficulties, some of which turned upon new discoveries concerning man himself. It was becoming apparent gradually that human beings, in addition to their ordinary mental life, possess a multitude of nervous activities, some of which are subconscious and many of which have no direct relation whatever with consciousness. The more these matters were looked into the more evident it became that our conscious activities were limited to a special part of our nervous organization, to the brain and perhaps even to the cerebral cortex, and that much of our nervous system had to do with operations quite free from conscious complications. Thus the heart, the blood vessels, the digestive tube, and other like parts, all of which possess their own nervous equipment, exhibit a range of operations of a highly complex and responsive kind that may be entirely dissociated from our conscious states. As these operations are directed toward the successful continuance of life of the individual in which they occur, we are forced to ask the question, May they not afford an example of the kind of nervous life led by many lower animals whose whole nervous equipment may then be as devoid of the so-called higher nervous states as our heart or our intestines are? An animal thus organized would be merely a delicately adjusted creature without desire, memory, or volition, but responding to changes in its surroundings with as much certainty and precision as our heart or digestive tube does to

its environment. To this question much of the evidence of recent years seems to be shaping an affirmative answer.

One of the newer lines of evidence touching on this point has to do with sense organs. These organs are usually regarded as bodily parts concerned with providing us with the elements of information as to the world about us. They are thus intimately associated with our central nervous activities. But they are known to occur in many lowly organized animals, such as the jellyfishes and the like, in which there are no central nervous organs appropriate for such information. In these animals the nervous impulses from the so-called sense organs pass directly to the muscles without first making their way through a central nervous organ. They serve merely as a means of exciting muscular activity and are concerned in no way at all, so far as one can judge, with sensations. Their action is comparable to that of our eye, which, when brightly illuminated, so responds that the muscular sphincter in the iris contracts, thus reducing the size of the pupil. With us sense organs have two functions. They deliver impulses that excite muscles to action, as in the instance just given, and they deliver impulses that serve our central organs in an informing way. Of these two functions only the first is possessed by many of the lower animals. Hence it is without doubt the more primitive, for the second function could not have arisen before the development of a central nervous organ, a part which, as already intimated, is absent from many simple animals.

To the older naturalists the presence of a sense organ was sufficient grounds for assuming that the animal experienced sensations characteristic of that organ. Thus the recognition of eye spots in jellyfishes was supposed to justify the opinion that these animals could see. But from the standpoint of the more recent work the presence of such an organ merely means that the animal is especially responsive to light, not that it has the sensations of sight, for the nervous strands from the eye spot in the jellyfish lead directly to the muscles and not to a central nervous organ. Hence the so-called sense organs of the lower animals, since they are in no necessary way concerned with sensations, are more correctly designated as receptors in consequence of their relation to the stimulus.

Since the so-called sense organs of many of the simpler animals are merely devices for exciting muscle to action, and since many of these animals possess no true central nervous organs, the responses of these admitted, and yet it is also equally clear that these higher animals have been derived from stocks that were purely tropic in their day. How have the tropisms disappeared from these lines of descent and what are the forms of response that represent the transition between tropisms and the diversified movements of the more complex animals? Some insight into the answer to this question can be gained by a comparison of phototropism and vision as elucidated through the retinal image.

It is now well recognized that many of the simpler animals, unicellular as well as multicellular, are extremely responsive to light. The amœba creeps away from a source of illumination, hydra creeps toward it, sea-anemones are for the most part photonegative, earthworms are positive to weak light and negative to strong light, and so forth. In all these simpler animals the surface of the given form is apparently open to stimulation by light in the same sense that our whole skin may be stimulated mechanically for touch. When light falls on an amœba, the formation of pseudopodia ceases on the illuminated side and continues on the side in shadow; hence the animal creatures are of a relatively restricted and circumscribed kind and lack the variety and spontaneity of the reactions of the more complex forms. Such restricted responses are represented by forced movements, or more particularly by tropisms, reaction types characteristic in general of the simpler animals and consisting of rather direct responses of the organism as a whole by moving either toward an obvious source of stimulation or away from it. Such responses, as Loeb has abundantly shown, are the usual types of movements for plants and the lower animals, and, though there is much difference of opinion as to the way in which a tropism is accomplished, there can be no doubt as to its predominance among the reaction forms of the simpler animals.

If, then, tropisms are the common types of response for the simpler animals in which receptors are directly connected with muscles, or at most connect with them through a very primitive kind of

central organ, it follows that this type of reaction must underlie that of the more differentiated animals and must have been gradually replaced by the kind of operation that we regard in ourselves as spontaneous and volitional. That there is almost nothing in the responses of the higher animals that recalls a tropism is generally moves away from the light. When sea-anemones are illuminated on one side, the pedal waves begin on that side and spread across the foot to the opposite margin thus carrying the animal, without any previous adjustment to the light, away from the source of illumination. When an earthworm is exposed to bright light it gradually turns its anterior end away from the light and, thus directed, creeps over a negative course. In all these tropic responses the animal falls quickly into line through the influence of the stimulus on the general receptive surface of its body and, without the necessary recourse to specialized organs such as eyes, it takes a direction in relation to the source of disturbance. There is not the least reason to suppose, except possibly in the case of the earthworm, that these activities are indicative of any sensational or other central-nervous element whatever. They are comparable with the movements of our internal organs, such as the heart and the intestine, and from this viewpoint they stand at an equally low and primitive level. They are in every sense forced movements of the tropic variety.

Probably much the same condition obtains in those animals that are provided with the so-called eye spots. These are small photoreceptors found on various places in different animals. They occur on the edge of the bells of jellyfishes, at the ends of the arms of starfishes and around the aboral pole of sea-urchins, on the heads of many worms, of arthropod larvæ, and perhaps of some snails. In typical conditions they consist of a group of receptive cells sunk in an open cup of pigment, so that the receptors are accessible to light only from a generally restricted region, the light from the rest of the field being received by the outer walls of the pigment cup. Such eye spots are unprovided with devices for forming images, either pupils or lenses. Occasionally lenses are present, but when such is the case, the lens is concerned with the concentration of light and not with the formation of an image. Such organs have been appropriately called

euthyscopic or direction eyes for the reason that they have to do with light only so far as the direction of its source is concerned and not with the possible formation of images. Animals that possess this type of photoreceptor to the exclusion of other types exhibit perhaps the most striking of all instances of phototropism. The larval stages of many insects are excellent examples of this kind. The maggots of blowflies creep with great precision away from a source of light or take a balanced course between two lights of different intensities or at varying angles of incidence. When one photoreceptor is covered, circus movements result. In short, the animal possessed of direction eyes shows a phototropism that is probably purest in its type in the sense that it is least complicated by extraneous factors.

From the direction eye as a point of departure two chief types of eyes have evolved, both characterized by the capacity to form images. Hence they have been called eidoscopic or image eyes. On the one hand, photoreceptive units, each more or less like a direction eye, have become arranged as a spherical system, thus giving rise to the compound or mosaic eye so common on the optic stalks of crustaceans and on the heads of insects. On the other hand, by enlarging the cavity of the direction eye and providing it with a wall, and a pupil or lens, or both, a camera eye has been produced such as is seen in many snails and higher mollusks like the squids, devilfish, and so forth, and in the vertebrates from fish to man. These two types of eyes produce images that are often remarkably rich in detail, the image in the compound eye being upright and that in the camera eye inverted.

When the light reactions of animals that possess compound eyes, like the insects, for instance, are studied, they are found to be by no means simple tropic responses. The mourning-cloak butterfly, when liberated in a room illuminated by a single, bright light, flies toward the light and behaves in a way to justify the designation positively phototropic. When, however, it is watched in the open field, its reactions are very different. After flying about in the sunlight for a while, these butterflies come to rest definitely oriented to the direction of the sun's rays, but instead of being headed toward the sun, as a positive animal should be, they head away from the sun in the

position of negative phototropism. Here, then, is an animal that in flight is positively phototropic, but in its resting posture is negatively so. These two activities, however, are intimately associated with the animal's environment. The flight toward light carries it under natural conditions to sunlit districts, and its negative position when resting in sunlight enables it to display its colors, which in the act of mating is a very important and significant step, as any one can observe in the open field at the appropriate time of year.

Not only is the phototropism of the mourning-cloak butterfly complicated, but the insect exhibits also this peculiarity: that though positively phototropic when in flight, it does not fly toward the sun, the source of strongest light in its natural environment. An experimental test of the animal from this standpoint shows that when it is placed midway between two sources of light of equal intensity, one a small point and the other a large surface, it regularly moves toward the large surface. Under like conditions animals without image eyes keep an even course between the two lights. For the butterfly with eidoscopic eyes the large area of less bright light determines the direction of movements rather than the small area of intense light. Hence in nature these animals fly from one patch of sunlight to another rather than toward the source of all light, and thus they may be said to prefer a place on earth to one in the sun. It takes only a moment's consideration to recognize how complicated the light responses of this butterfly are as compared with those of a purely phototropic animal.

That the reactions of insects to light are built up on a background of phototropic activity seems to the writer to be perfectly clear. The pure phototropic responses are often strikingly exhibited in the larval stages where only direction eyes are present, a condition of affairs pointed out long ago by Loeb in the caterpillars of the Porthesia moth. But they are also easily disclosed in the adult condition, where they are covered at most by a veneer of instinctive activities which represent in reality modified tropic movements such as have been pointed out in the mourning-cloak butterfly. Thus Dolley (1916) has shown that even in the mourning-cloak butterfly itself circus movements may occur on blackening one eye, and the same is true of the still more

complex honey bee as studied by Minnich (1919). And Garry (1918) has recently demonstrated in a most striking way the tropic nature of the pose and locomotion in certain flies. Thus the adult insect, though subject to the most diverse movements in an illuminated field, has underlying its whole system of response a basis of simple phototropism. This relation is nowhere better illustrated than in the blowflies. The maggots of these flies are strongly phototropic in a negative sense and exhibit those balanced reactions to opposing lights that are characteristic of the purest form of phototropism. They possess eyes, but these eyes are little more than direction eyes. When they emerge as adults, they have well-developed compound eyes. Under laboratory tests they are said to be positively phototropic, but in the field they exhibit such a variety and complication of photic response as to recall the state of the mourning-cloak butterfly. Bees are without doubt positively phototropic, but their daily life in the illuminated field in which they live is as complex in many respects as that of a human being. As von Frisch (1914) has recently shown, they can be taught to associate color with food supply, and it is impossible to explain their homing instincts without assuming memories, visual and otherwise, of an order fairly comparable with those found in the vertebrates. Thus many insects, though fundamentally tropic in their underlying nervous organization, have built upon this organization an immense superstructure of reaction types mostly of an instinctive kind that obscures and hides the original simple tropic scheme. This overgrowth in phototropism is dependent upon, first, the development of an eidoscopic eye whose image is rich in detail, and, secondly, upon the development of central nervous organs capable of caring for such detail. In this respect the insects offer remarkable transition forms between the purely phototropic simpler organisms and those in which phototropism seems to have vanished completely.

It is a fair question to ask whether vertebrates exhibit any tropic responses whatever. Most students of this subject would answer this question, I imagine, in the negative. Yet it is very difficult to explain, for instance, the feeding habits of the dogfish without assuming a tropic basis. When hungry dogfishes are liberated in a pool

in which food is hidden, they begin sweeping the bottom in rapid circular movements, turning now to the right and now to the left. If both nostrils in the fish are closed with plugs of cotton, these movements do not occur. If, now, one nostril is freed, the circular form of locomotion returns with this peculiarity, however, that the circles are now almost always in one direction—*i.e.*, with the free nostril toward the center. Plainly the dogfish scents its food and in hunting turns, as animals exhibiting tropisms do, in the direction appropriate for the more intense stimulus. Thus the dogfish shows responses that in every way have the earmarks of a tropism. This condition, however, is very exceptional, for in general the responses of vertebrates to their environment, as every one knows, resemble vastly more those of the more complex insects than they do the tropic reactions of the simpler organisms.

A remarkable form of vertebrate response in this particular is the instinct shown by newly hatched loggerhead turtles to go toward the ocean. It is a most singular spectacle to see a dozen or more of these newly hatched creatures scramble across the horizontal surface of a wharf directly toward the water which, in consequence of a raised wooden edge, they could not see and with which they had had no previous experience. What determined their direction of motion was at first sight very difficult to say. After some trials, however, it was found that they commonly went away from any large diversified mass, especially when it occupied a part of the horizon line, and they as commonly went toward a uniform and uninterrupted part of the same line. Their first steps in this operation were extremely interesting to watch. When a young turtle is placed in position to move, he quickly raises his head, makes a complete turn through a whole circle to test out apparently his surroundings, and then takes a straight course toward the clearest part of the horizon. That this reaction has of necessity nothing whatever to do with the ocean can be shown by starting the turtle near some high shrubbery, but on the side away from the sea and toward a free, open field. The animal will then move away from the shrubbery and toward the open field with as much certainty as it had previously done toward the water, though in this instance it is plainly moving away from the element which ordinarily it would be expected to seek.

The natural response of the turtle to the sea is so obviously instinctive and so uniform that it presents all the superficial traits of a tropism, but when it is looked into, it appears to be a very precise form of instinctive reaction to the details of the retinal image. When loggerhead turtles were tested in a dark room provided with a single light, they went neither toward the light nor away from it, but remained for the most part quietly resting where they had been put. Contrary to the view expressed by Hooker (1911), they are not phototropic. They are active when their retinal fields are full of detail and they move toward that part of the field in which the horizon is most open. Under natural circumstances this usually brings them to the sea, but it does not necessarily do so, and it is in no sense a true tropic reaction. The young loggerhead turtle exhibits, then, an activity that superficially resembles a tropism, but that in reality is very different. In this respect the animal declares its higher nature.

Most vertebrates respond in very precise ways to the details of their retinal fields. Thus frogs and toads will seize and swallow almost any small moving object, be it a pebble or a bit of wax attached to a string, or a living insect. The motionless insect, like the motionless pebble, escapes. It is something moving in a field otherwise quiescent that excites the reaction. This reaction is dependent, therefore, on a detailed retinal image associated with a highly differentiated central nervous apparatus.

By a strange coincidence a frog through a simple operation may be reduced from an animal responding in the highly complex way just described to one that reacts after the style of pure phototropism. Frogs, like most other animals of their class, are sensitive to light through the skin. If the anterior part of the head of a frog is cut off transversely just behind the eyes, the operation deprives the animal at once of retinal images and of its higher nervous centers. What is left of the animal still responds to light, but only through the skin and by means of a much simpler central apparatus than it had before the operation. Such a frog will maintain a natural sitting posture, and, if near a window, it will turn till it faces the light, after which it will commonly move forward from time to time toward the window. It is in no way excited by small moving objects about it,

but it presents all the appearances of a simpler positively phototropic animal. Its transformation is most perfect and complete. Here, then, the influences that cover over and obscure the fundamental tropisms have been removed and the animal is reduced to that state which in a way was probably characteristic of its remote ancestry. Thus by a simple operation a highly complex vertebrate may be reduced to a simple tropic animal.

If this outline represents the true course of events, it follows that vertebrates react in ways other than tropic in consequence of their enriched sensory fields, whose details are relatively enormous as compared with those of the simpler animals. This is especially true of the retinal fields. Such enriched sensory relations have induced in these complex animals the development of a vastly intricate central nervous organ, and on these two elements, the complex field and the intricate center, are based the possibilities of the sensations, memories, volitions, and other like activities that give diversity to our performances as compared with those of the simpler animals. Though vertebrates show little of the primitive tropic responses, the insects afford interesting examples of balanced forms of behavior in which, though the tropism is clearly discernible, the higher type of response, the response to detail or what may be called the singular response, is clearly visible.

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SOME TOPOGRAPHIC AND CLIMATIC CHARACTERS IN THE ANNUAL RINGS OF THE YELLOW PINES AND SEQUOIAS OF THE SOUTHWEST.

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(*Read April 21, 1922.*)

The material here presented is part of a long-continued investigation of historical data contained in the annual rings of trees. Such data naturally are impressed by the environment, and the factor of the latter which interests us most is the climate. In this study as a whole, some 500 trees have been used, scattered in groups from California to Austria. About 110,000 rings have been dated and measured. The conclusion hitherto reached may be stated in a few sentences. First: the rings in the groups studied may be dated with practical certainty. This is not intended as a general statement for all trees in the world, because the identification of rings seems to increase in difficulty as the snowfall of winter decreases. Second: the yellow pines in the dry climate of northern Arizona give in their rings a rainfall record of considerable accuracy, namely, 70 per cent. in groups of trees near the rainfall record station. This is increased to an accuracy of some 85 per cent. by the application of a simple formula for conservation. Third: certain groups of wet-climate trees, especially about the Baltic Sea, give a very exact record of solar activity, as indicated by the relative sunspot numbers. Fourth: the rings in certain wooden beams used in prehistoric construction can be made to give us certain chronological facts. For example, it has been shown by this means that the old ruin of Pueblo Bonito in northwest New Mexico is forty to forty-five years older than that of Aztec, some fifty miles north of it. Fifth: three mechanical aids have been developed, a tubular borer for securing a core extending from the outside to the center of a beam or a tree, a recording microscope slide or micrometer for measuring ring widths, and a cycloscope for rapidly determining periodic effects in a plotted curve. By this

last instrument as many as 34 curves of 500 points each have been tested in one day for all periods between 5 and 32 points; in this the curves were taken entire or in any number of parts.

At the present development of the investigation a review of the topographic effects observed in the trees seemed necessary. For such purpose a group of some 21 sequoias which had grown in Redwood Basin, 15 miles east of General Grant National Park, Fresno County, California, was used. It is understood that these trees had been cut down, and that radial pieces were cut from the stumps, shipped to the laboratory at Tucson, and there identified and measured. The trees were scattered for a mile along a valley whose steep slope was toward the north. The upper end is near the top of the mountain, but a spring supplies a small stream of water. The upper trees mostly had a very dry soil, while those below, some 600 or 700 feet in vertical measurement, had more level ground and greatly increased moisture. The average growth per century in the last 500 years was about 7.6 cm. The least was less than 4 cm. and the greatest was over 15 cm. The big-growing trees were mostly close to the water-course in the lower basin. The average growers were mostly around the edges of the basin, while the slow-growing trees were chiefly at the tops of the slopes. All this was as expected. Three larger growing trees close to the upper limit formed interesting exceptions. One was an infant sequoia, only 700 years old when cut, and therefore naturally a fast-growing tree. Another at the very highest point was about fifty yards above the spring and undoubtedly tapped an underground flow of water leading to it. Its type of rings was very similar to those in the basin. The third exception had very large rings, but they were full of sensitive variations like the slow-growing trees near by. That tree is probably over a pocket of water whose help increased its growth, but which failed in extremely dry conditions. It is evident, then, that with the sequoias moisture may control the growth up to a maximum fully four times as large as the minimum.

The type of ring and its adaptation to identification and study varies greatly with the moisture supply. The large rings of the quick-growing trees are either very complacent—that is, of the same

size for many years in succession—or gross in character, which means extraordinarily large rings here and there and the whole grouping apparently subject to slow surges in size as one glances across the sequence from center to bark. Gross rings in one tree have about an equal chance of appearing or not appearing in any other tree near by. Since gross and complacent rings have little individuality, it is not always easy to identify their dates, especially if the outer layers of wood have been cut away as was usually done in felling the sequoias. On the other hand, the slow-growing, low-moisture trees are full of snappy irregularities which may be found in tree after tree, thus rendering accurate dating a remarkably easy process. It is also immediately evident that these latter sensitive trees give short-period variations far more accurately and effectively than the complacent trees.

A study of cycles of growth in the last five centuries of these twenty-one trees shows that often basin and upland trees vary together, and that in comparison with the others the well-watered trees show no lag of more than three years. Certainty in regard to no lag at all has not yet been reached.

Out of these latter tests has come the most interesting fact of all to students of cycles. The yellow pines of northern Arizona, much more sensitive than the upland sequoias, show the history of the eleven-year sunspot cycle in a prominent manner, even though other cycles are present. The fairly sensitive upland sequoias show the sunspot cycle, but other cycles are more prominent, and the eleven-year period has to be traced in multiples or harmonics to overcome the various interferences. But the complacent rapid-growing sequoias show the sunspot cycle only here and there, and so far no certain way has been found of using them in studying the history of that cycle.

This brings us to the consideration of the cycles in tree growth as climatic products. The ring itself is a result of the seasons. Variations in the rings in dry climates are found to match the rainfall. But the rings display marked cycles, and if these also can be interpreted as climatic, they are likely to prove of great assistance in studying climatic variations, because they stretch over great historic

periods. The first line of evidence naturally is to compare the cycles in a tree sequence with cycles in rainfall records near by. This is done successfully with the Prescott trees, but the period over which such a comparison can be made is under half a century, and that is too short for satisfactory results. These trees show a 7.7-year cycle and the eleven-year sunspot cycle. The rainfall shows the sunspot cycle and a 7.7-year cycle, but the latter could be interpreted as anything between 5.5 and 8 years.

The next line of evidence depends upon the area over which cycles may be traced, for the common environment over a large area is climate. A test has been made between ten pines in southwestern Colorado, nine pines in northwestern New Mexico, fifty miles away, and nineteen pines at Flagstaff, Arizona, 200 miles southwest of the other groups. These three groups are largely identical in their cycles for the last two hundred years or more. This gives us much confidence that these cycles are real and are climatic in origin.

But still further evidence comes from a purely historical source and is of a kind full of interest on its own account. Professor E. W. Maunder, of England, in a recent letter, called attention to the prolonged dearth of sunspots between 1645 and 1715 and judged that if there were a connection between solar activity and the weather and tree growth, this extended minimum should show in weather conditions and in the trees. On receipt of the letter this period was immediately recognized as the interval in which the greatest difficulty had been found in tracing solar effects. In fact, in 1914, when the writer was trying to trace the history of the solar variations in the yellow pines, the difficulty between those dates almost led to the view that the trees were not giving this cycle. A present review of the eleven-year period in those trees confirms its well-marked existence from before 1400 to the middle of the seventeenth century. Soon after 1700 it reappears, but not in complete form until the latter part of that century.

The test was then carried to the sequoias and two difficulties were encountered. First, it was found that the slow-growing, sensitive upland trees were the ones which displayed the solar cycle, and, second, the interference by other cycles was such that the double

period of about twenty-three years was a more satisfactory means of tracing the vicissitudes of the solar period. When these conditions were observed the same result was obtained as before from Arizona. The twenty-three-year period, in fact, begins to show change about 1635 instead of 1645 and continues on a ten-year cycle to the neighborhood of 1712, when the double sunspot period is resumed. Probably more and more evidence will be brought to bear on this point. Almost at the time of writing it is noticed that the Vermont hemlocks show a ten-year period from their beginning in 1654 to well on in the middle of the next century. The eleven-year period begins to show at about 1700 and becomes dominant in the latter part of that century. Modifications will doubtless be made in historical review of evidence in the trees of the prolonged dearth of solar influence at that time, but the evidence, so far as it goes, is wholly in favor of a pronounced effect in the growth of trees.

This correlation found in response to Professor Maunder's note therefore led to two results. First, it seemed to confirm strongly the idea that the cycles in the trees are not merely real, but they are related to weather elements and to cosmic causes; and, second, it gave added weight to the provisional history of solar variation derived from a study of the 3,200 years of sequoia growth. There has not been enough time yet to review that large mass of measures and derive a satisfactory history, but in conclusion a brief memorandum upon that point will be of interest. It is probable that from 1300 B.C. to well after 1000 B.C. the sunspot cycle was well developed; then it slowly decreased. From 300 B.C. on, it was increasing and was very conspicuous during the first two centuries of our era. Then it decreased and from 400 to 650 A.D. was only occasionally evident. From 650 to 850 or 900 it seems fairly continuous. Then it appears only occasionally until about 1250, when it again became fairly continuous with the changes in the seventeenth century above noted.

Thus there seem additional grounds for regarding the trees as supplying useful historic data and giving us long ranges of time over which to study the vagaries of our fickle climate.

In summarizing one notes a strong topographic effect in the trees of the Southwest, as expected; the maximum growth in well-watered

ground is four times the minimum in dry ground and is accompanied by profound differences in type of ring; the eleven-year solar period (of the double-crested, dry-climate type) shows with rapidly increasing distinctness as one passes from the complacent, moist-ground trees of the basins to the very sensitive, dry-soil trees of the uplands and of Arizona. The climatic feature considered in this paper is the reality of certain possible climatic cycles found in the trees. That these cycles are real is attested by the extent both in time and space over which they are traced. This conclusion enables us to trace in the rings of the sequoia a provisional outline of solar variation for the last 3,000 years.

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THE EFFECT OF DIURNAL VARIATION OF CLOCK RATES UPON LONGITUDE WORK.

By R. H. TUCKER, C.E.

(*Read April 21, 1922.*)

In dealing with the question of a diurnal variation of clock rates it may be necessary to introduce some reservations—to employ the language of recent diplomatic conferences.

These reservations are mainly covered by treating the phenomena as those of observation, and still further limiting the data of observation to the meridian circle transits of stars.

Either our clocks run faster at night or there are systematic corrections to our observations that have not been detected nor applied.

Such systematic corrections might be due to errors of observation and reduction, or to some periodic term affecting the position of the meridian.

The hourly rate of a clock, computed from transit observations during any period of a night, will always differ from the average hourly rate during a period of one day unless, by a rare chance, the accidental error of observation exactly balances the error of the adopted right ascensions.

These two classes of error are of nearly the same order of magnitude as regards their accidental character.

For instance, the right ascensions of NEWCOMB, as tabulated in the *American Ephemeris*, have average accidental errors of at least $\pm 0^s.02$ per star.

The probable error of an observation with our instrument is quite precisely $\pm 0^s.02$.

With any number of stars used, the probable error of an observed clock correction, and that of an observed rate, would be made up of virtually equal errors of observation and of right ascension. Of yet greater importance in deriving hourly rates are the systematic errors of the adopted right ascensions, since these can not be diminished by increasing the number of stars observed.

Both classes of error can be eliminated by observing the same stars in each hour of right ascension, and closing a cycle of observations in which every hour has been included. Also some systematic errors of observation, such as that due to magnitude equation, are eliminated in the cycle.

The average systematic error of NEWCOMB's right ascensions at this date is not far from $\pm 0^s.02$ per hour.

The average difference between two consecutive hours is smaller than the average per hour, since the systematic errors are periodic in character, approximately of the form, $-0^s.02 \cos \alpha + 0^s.01 \sin \alpha$. A comparison of the right ascensions of NEWCOMB in the *American Ephemeris* with those of Boss in his *Preliminary General Catalogue* gives an indication of the character of the systematic errors to be anticipated.

For 325 stars at present under observation here, between 37° north declination and 30° south, the average difference is $\pm 0^s.022$ per star. In hourly groups the average difference is $\pm 0^s.006$ per hour. Differences between individual stars are evidently mainly fortuitous.* This does not imply an absence of systematic errors, but does indicate that the systematic errors of the two authorities are similar. The average difference between two consecutive hours is $\pm 0^s.010$ for the two lists.

If we use the right ascensions of Boss, the computed hourly rate of a clock will differ $0^s.010$ from the hourly rate computed with NEWCOMB's right ascensions, in the average, and may differ more than twice that amount. The right ascensions of Boss appear to have the relative weight 3 : 1, as regards accidental errors, and weight 2 : 1, for systematic errors.

The later fundamental system of AUWERS appears to be as precise as that of Boss, both as regards accidental errors and systematic errors.

NEWCOMB's system antedates the other two by about ten years. If we include as clock stars only those within 15° of the equator, 317 in number, the average differences between NEWCOMB and Boss are

* The difference $0^s.006$ for an average of 13 stars per hour corresponds to the average difference of $0^s.022$ per star.

nearly the same as those above, but the mean difference is $B-N=-0^s.009$, while the mean difference of the more widely extended list is less than $0^s.001$. Many of these stars close to the equator were not included in our observing program, since the intervals between successive stars were often too short.

Boss P. G. C.—Newcomb A. E.

R. A.	Stars	$\Delta\ \alpha$	Diff.
0	11	$-0^s.003$	
			$0^s.018$
1	13	+ 015	
			015
2	15	000	
			016
3	15	− 016	
			012
4	13	− 004	
			005
5	16	+ 001	
			005
6	12	− 004	
			003
7	14	− 001	
			011
8	14	− 012	
			025
9	11	+ 013	
			001
10	11	+ 014	
			015
11	15	− 001	
			008
12	11	+ 007	
			002
13	11	+ 005	
			013
14	13	− 008	
			008
15	15	000	
			005
16*	13	− 005	
			004
17	9	− 001	
			003
18	15	+ 002	
			005

19	13	— 003	
			016
20	15	— 019	
			022
21	14	+ 003	
			011
22	16	— 008	
			013
23	18	+ 005	
			008
Average	13	± 0.006	± 0.010

From our observations extending over a period of a quarter of a century the mean excess of the hourly rate at night over the average hourly rate during one day is $0^s.006$. This corresponds to a variation in the daily rate of an amplitude of approximately 0.3 of a second. The difference between observed and interpolated clock corrections would be a maximum for an interval of six hours, and would amount to over $0^s.03$. Double this difference would occur between the observations near sunset and sunrise, and an observed difference of $0^s.06$ has been found between clock corrections at those epochs of the day. These numerical results are still subject to revision, as more precise values are to be anticipated from our current series of observations.

In fundamental right-ascension observations differences of this size should occur, but the alternate observations of groups of stars, twelve hours apart, has smoothed out this effect in our adopted systems.

It is not often necessary to carry the daily rate forward more than two hours except in fundamental work, and the difference between daily and hourly rates would rarely introduce an error exceeding $0^s.01$.

It has been our custom generally to adopt the hourly rate derived during the period of observation, when that period is of sufficient length, in reducing transit observations. The results thus obtained conform to the adopted right-ascension system, with its errors included. The actual performance of the clock has been of secondary importance in deriving the right ascensions.

* [one star] ω *Herculis*, mistake in P.M.

Changes of temperature in our well-protected clock cases have had no sensible effect upon daily clock rates for short periods, such as one day, with which we are here concerned.

Ordinarily the range is less than one degree, and nearly always it is progressive, so that the temperature at night falls between those of successive days.

The variation of atmospheric pressure, as recorded by the barometer, affects the rate of a pendulum clock not hermetically sealed. For our Riefler clock, installed in 1907, a change of one inch in atmospheric pressure changes the daily rate $0^s.46$.

In our fine summer weather the average barometer reading at midnight is 0.04 inch below that at noon, and the lowest reading commonly occurs in the early morning, following midnight. In fine weather in winter the reading at midnight is 0.01 inch below that at noon, and the lowest readings occur in the afternoon.

The summation of the hourly excess at night has been divided into two periods, corresponding to the use of the Dent clock, unsealed up to 1907, and the use of the Riefler, following that date.

The first period gives a mean hourly excess of $0^s.007$, and the second period gives $0^s.004$. The difference between the two results and the mean results are too large to be accounted for by the variation of the atmospheric pressure.

The observation of clock corrections and rates during the night hours should be uninfluenced by any possible deviation of the meridian plane, due to barometric or thermal gradients in the atmosphere, such as might be suspected at sunset and sunrise.

The effect of such gradients has been found to be very small, even at these epochs of greatest disturbance.

Excess of Hourly Rates at Night.

Series	$\Delta\rho$	Wt.
1893 to 1894	$-0^s.0097$	2
94 " 95	-0.0059	2
95 " 96	-0.0066	1
97 " 98	-0.0039	2
98 " 1900	-0.0076	4
1901 " 04	-0.0086	4
05 " 06	-0.0052	2
06 " 07	-0.0047	2

07 " 08	—0.0037	1
14 " 15	—0.0038	3
17 " 18	—0.0047	3
1920	—0.0037	3
Mean (12)	—0.0057	
	±0.0004	
Weights	—0.0059	

The weights have been assigned according to the number of nights per year, and the number of stars per night.

Small undetected progressive changes in the position of the instrument would be represented in the computed clock corrections and rates. The changes in instrumental corrections were usually measured over a period of at least four hours.

It is hardly credible that uniform progressive changes would persist undetected in this long term of years if they were of sufficient size to account for the hourly rates as observed.

To explain the sunset and sunrise results, systematic differences would need to be of a decided character. Instrumental corrections and the indications given by the mire readings have had careful scrutiny in this connection.

Physical or mental fatigue might be presumed to affect the personal equation of the observer, and thus influence the computed clock rate. The effect would more probably produce erratic results, with larger accidental errors of observation.

The reaction times at sunset and sunrise would necessarily be of quite different character, also, from those at night to make plausible this explanation. Our current series of observations will give a test of such a possible effect, as we shall have mean hourly excesses during periods of six consecutive hours, in each of which the systematic errors of right ascension will have been eliminated.

A diurnal term in longitude, similar in character to the fourteen-month variation, would produce a diurnal periodic variation in clock corrections and rates, as observed. If the maxima occur at sunset and sunrise, the most rapid changes would occur at noon and midnight.

Clocks do not run over long periods of time with the uniformity requisite to test the fourteen-month term, and we derive the long-term variations in the longitude from the corresponding observed

variations of latitude. But any good clock can be relied upon during a period of one day to test a diurnal variation.

If there is a physical cause for a diurnal variation in our observed results, the best clocks will give the best defined variations.

Since latitude observations with the zenith telescope have been confined to the night hours, we can not expect much contribution to the solution of a diurnal term from that delicate differential instrument. If the maxima of longitude variation occur at sunset and sunrise, the maxima of latitude variation should come at noon and midnight.

The current observations at the international zenith telescope stations are made in two groups, at nearly equal intervals each side of midnight.

The closing error of the groups, which is about 0".2 distributed among twelve periods, might be due to a variation with a daily maximum that does not fall exactly half way between the two daily groups.

At your neighboring institution, the Flower Observatory of the University of Pennsylvania, a distinct difference in latitude results was derived by Prof. C. L. Doolittle between early and late hours of the night. This difference could be attributed to an error in the adopted constant of aberration, and a correction (0".08) was computed by that most thorough and capable observer. It will be recalled that the zenith telescope observations have pretty uniformly given larger values for the constant of aberration than those derived from other sources. With the value of the constant, 20".47, only one quarter of the difference derived by Prof. Doolittle would be accounted for.

Observations with the prime vertical instrument by M. Jean Boccardi, at the Turin Observatory, in 1920, were designed to show a differential effect in latitude results during the night hours.

By comparing the observed differences between stars separated about three hours in interval, through a cycle of nearly one year, he derived a cosine term with an average coefficient of 0".07.

The extensive series of prime vertical observations of α *Lyræ* at the U. S. Naval Observatory, seventeen hundred observations in

nineteen years, exhibit a difference of 0″.5 between declinations measured by day and night.

Corrections to the adopted constant of nutation were computed from this series, from both day and night observations combined, and from the two periods of the day separately.

The first of the solutions mentioned gives a correction of + 0″.03 to the constant 9″.22.

To return to meridian circle results, our fundamental work during the years 1905 to 1908, and in 1916, has given us the observed latitude at all hours of day and night.

Over one thousand observations have been combined in deriving the following diurnal term. More than one quarter of the total number are of zenith stars, close enough to the zenith point to be observed facing either north or south for the measurement of bisection error.

A somewhat larger number are of stars bright enough for daylight observation, divided into groups for which the means are close to the zenith.

The stars *α Andromedæ* and *Polaris* furnish a third of the total number.

Observations of *Polaris* and *β Ursæ Minoris* are the only ones for which corrections for the diurnal variation of refraction are of importance. This correction has been derived from observations of stars at large zenith distances on both sides of the zenith, and the solution is independent of the latitude and its variation and of the nadir readings. The diurnal variation in the atmospheric refraction at this station is approximately one per cent. of the total refraction. A separate solution for the diurnal term had been made from the zenith stars only, before including the results from the other stars. Errors of refraction could play no part in this solution, which gave the same numerical coefficient as the solution from all the stars, within one unit in the third place of decimals.

The observations are nearly evenly distributed between daylight and night hours.

All have been corrected for the latitude variation of long periods from the results of the international zenith telescope stations, excluding the z term.

The diurnal term given by these observations is $+0''.14 \cos T$, where T is reckoned from noon. The solution gives also a small sine term, with coefficient $0''.03$, which does not appear to be distinct enough to adopt. The cosine term is less than one half the difference in the observed clock corrections at sunset and sunrise. It is of the same size approximately as that of the fourteen-month term of the latitude variation.

Since the axis of figure of the earth does not coincide with the axis of rotation of the earth, the pole of figure makes a daily revolution about the pole of rotation. The pole of figure advances only $0''.8$ on its curve, representing the fourteen-month rotation, so the daily revolution will be nearly circular.

If the deviation of the two axes is constant during a day, there could be no resulting variation of latitude, according to the accepted definition of that coördinate, assuming that the axis of rotation does not shift its position during the same period.

If the position of the instantaneous axis of rotation of the earth with respect to the celestial sphere still requires correction, depending upon an error in the adopted constant of nutation, these several anomalies of observation may possibly be reconciled with theory.

As observations of this character have served to determine our astronomical constants, modern refined observations may indicate a need of revision of the values.

The test should be sought in fundamental observations with the meridian circle, since the full amplitude of variations can be more effectually observed, while other classes of observation have given mainly tests of differential changes.

Since aberration has minimum effects upon transits of stars near sunset and sunrise, and also minimum effects upon zenith distances of stars near noon and midnight, the two diurnal variations in our results do not appear to indicate any correction to the constant of aberration.

The diurnal term in observed ϕ may indicate a small correction ($0''.02$) to the constant of nutation.

The solution of a diurnal term is commonly involved with that of an annual periodic term, when dealing with observations of any

star at all hours of the day, but the observations of *Polaris* include many consecutive transits at opposite culminations.

The variation in clock corrections or rates is distinctly of a diurnal character.

DIURNAL TERM IN LATITUDE VARIATION.

Epoch		$\Delta\ \phi$	Comp.	O–C
0h.6	P.M.	+0".06	+0".14	−0".08
1 .4	"	+0 .07	0 .13	−0 .06
2 .5	"	+0 .03	0 .11	−0 .08
3 .4	"	+0 .08	0 .09	−0 .01
4 .4	"	+0 .04	0 .06	−0 .02
5 .5	"	−0 .30	+0 .02	−0 .32
6 .6	"	+0 .26	−0 .02	+0 .28
7 .3	"	−0 .08	0 .05	−0 .03
8 .5	"	−0 .07	0 .09	+0 .02
9 .4	"	+0 .02	0 .11	+0 .13
10 .4	"	−0 .15	0 .13	−0 .02
11 .6	"	−0 .10	0 .14	+0 .04
0 .4	A.M.	−0 .30	0 .14	−0 .16
1 .5	"	−0 .34	0 .13	−0 .21
2 .4	"	−0 .14	0 .11	−0 .03
3 .3	"	−0 .01	0 .09	+0 .08
4 .3	"	+0 .09	0 .06	+0 .15
5 .2	"	+0 .01	−0 .03	+0 .04
6 .5	"	+0 .12	+0 .02	+0 .10
7 .4	"	+0 .09	0 .05	+0 .04
8 .4	"	+0 .10	0 .08	+0 .02
9 .3	"	+0 .11	0 .11	00
10 .5	"	+0 .26	0 .13	+0 .13
11 .3	"	+0 .14	0 .14	00
Mean		0 .00	0 .00	0 .00
Average		±0 .12	±0 .09	±0 .08

$$\Delta\ \phi = +0''.14 \cos T$$

The groups of $\Delta\ \phi$ have an average above 42 observations each.

The probable error of the night groups derived from individual residuals is $\pm 0''.08$, and that of the day groups is $\pm 0''.12$, average $\pm 0''.10$.

The two sunset groups have residuals of four times the average O–C, which would be $\pm 0''.06$ if these two results are combined into one.

Observations.

Zenith stars	258
α Andromedæ	135
Polaris	231
β Ursæ Minoris	77
Groups	319

The latitude from this partial summation of our fundamental observations is $\phi_0 = 37° 20' 25''.6$. This was the value derived from the first effective meridian circle work here. Previous to the beginning of that work, in 1893, the adopted latitude of the instrument, as furnished by the *U. S. Coast and Geodetic Survey*, was a full second of arc smaller. This difference could be due to the errors of declination of the stars employed in the earlier determination, and to the lack of corrections for the periodic variation of latitude.

As to possible sources of systematic error in our zenith distance observations, there might be a sensible difference between nadir readings during daytime and night.

This would probably not appear as a periodic term, however. All accidental errors, even those of circle readings, are larger in the daytime.

It is more difficult to concede the probability of a shift in the zenith point, due to a variation in the refraction at the zenith. Especially is this less probable during the night hours, when the atmospheric conditions are most stable.

Whether our observed periodic variation is in the clock rates, or in some term affecting the position of the meridian, the determination of the difference of longitude between two widely separated stations will show the effect if the phase is the same for both stations.

The usual procedure for an exchange of longitude signals is to observe the same list of stars at each station, in order to eliminate errors of right ascension.

If the stations differ 90° in longitude, the rate of one clock will be carried forward six hours to the epoch of observation at the other station.

If the established daily rate of the clock be used, the accumulated error would amount to $0^s.03$ in this interval.

Every exchange of signals would have this error, but if signals are sent each six hours during one day the errors would occur in pairs, two successive plus errors being followed by two minus errors.

The double amplitude of $0^s.06$ would occur in two consecutive exchanges, twelve hours apart.

If two stations differ 180° in longitude, exchange of signals every six hours would give differences alternately of $0^s.06$ and zero.

If we take an exchange of signals between Greenwich and a station 90° west, and the group of stars is observed at midnight, the derived difference of longitude would be $0^s.03$ in error, but no difference in the exchanges at both epochs of observation would appear. If the group of stars were observed at sunset at each station, the errors of projected clock rates would be of contrary signs, and there should be a difference of $0^s.06$ in the exchanges.

Δ Longitude 90°.

W.	Corr.	E.	Corr.	W.—E.	Change.
6 P.M.	$+0^s.03$	Midnight	$^s00$	$+0^s.03$	
					$^s00$
Midnight	00	6 A.M.	-0.03	$+0.03$	
					-0.06
6 A.M.	-0.03	Noon	00	-0.03	
					00
Noon	00	6 P.M.	$+0.03$	-0.03	
					$+0.06$
6 P.M.	$+0.03$	Midnight	00	$+0.03$	

Δ Longitude 180°.

W.	Corr.	E.	Corr.	W.—E.	Change.
6 P.M.	$+0^s.03$	6 A.M.	$-0^s.03$	$+0^s.06$	
					$-0^s.06$
Midnight	00	Noon	00	00	
					-0.06
6 A.M.	-0.03	6 P.M.	$+0.03$	-0.06	
					$+0.06$
Noon	00	Midnight	00	00	
					$+0.06$
6 P.M.	$+0.03$	6 A.M.	-0.03	$+0.06$	

Δ Longitude 120°.

W.	Corr.	E.	Corr.	W.—E.	Change.
6 P.M.	$+0^s.03$	2 A.M.	$-0^s.01$	$+0^s.04$	
					$-0^s.02$
Midnight	00	8 A.M.	-0.02	$+0.02$	
					-0.06
6 A.M.	-0.03	2 P.M.	$+0.01$	-0.04	
					$+0.02$
Noon	00	8 P.M.	$+0.02$	-0.02	
					$+0.06$
6 P.M.	$+0.03$	2 A.M.	-0.01	$+0.04$	

If the phase of variation has a constant epoch, the maxima would occur at the same instant for all stations, and the exchange of signals would exhibit no changes.

Without regard to the true difference in longitude, an exchange of clock signals at intervals of six hours through one day, between two stations 90° apart, may furnish a test of the phase of the variation, if it is in clock rates. The only necessary conditions are two clocks with well-established uniform daily rates and the requisite precision in recording the signals.

It is hoped, and even hopefully expected, that a transatlantic record of the wireless signals from Bordeaux may be employed for this purpose.

Our current series of observations should have a weight of ten per night, as compared with the earlier work tabulated above, since there are about eighty stars observed in each period of six hours, while most of the earlier work included but eight stars in periods of four hours.

Whatever the result, when we close the cycle of observations next September the weight will be assumed equal to that of all the preceding observations. This policy is justified by the rigorous attention that is being paid to the necessary details of program and of reduction. An interchange of stars or the loss of a single observation will always mask, if not mar, the effect of the small correction we are sifting out. Nevertheless there has been, personally, a convincing effect in the weight of evidence of the old work, from which these variations emerge as a by-product—to close with an industrial figure of speech.

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ECOLOGIC AND MORPHOLOGIC STUDY OF THE CLOVERS (TRIFOLIUM).

By JOHN W. HARSHBERGER.

(*Read April 21, 1922.*)

This study of the clovers was begun in 1907 and has been pursued intermittently ever since. Sufficient data has accumulated to warrant its assemblage for publication. The material for investigation was gathered in the open for all of our common species, numbering about six, and the remainder was raised from seeds planted in pots in the greenhouse. After considerable correspondence, which was seven years before the outbreak of the world war, seeds were obtained from Ames (Iowa), Amsterdam (Holland), Berlin (Germany), Besancon (France), Burlington (Vermont), Cambridge (England), Copenhagen (Denmark), Dijon (France), Dublin (Ireland), Hamburg (Germany), Innsbruck (Austria), Kew (England), Knoxville (Tennessee), Lincoln (Nebraska), Northampton (Massachusetts), Rome (Italy), St. Louis (Missouri), St. Petersburg = Petrograd (Russia), Tiflis (Persia), Tucson (Arizona), Vienna (Austria), Washington (District of Columbia). The seeds from Washington, D. C., were obtained from agrostologists connected with the United States Department of Agriculture and from the Bureau of Foreign Seed and Plant Introduction. Some seeds were purchased from dealers in New York and Philadelphia. The trial sowings of these seeds showed that most of them readily germinated, but some of them were refractory, or failed to sprout entirely. The seedlings and young plants intended for histologic investigation were fixed in chromacetic acid and finally put into 50 per cent. alcohol for preservation. The growing plants were used for experimentation on their leaf movements.

Number of Seeds in the Pods.

Some of the material received from botanical gardens had not been hulled and it was thought worth while to determine how many seeds

were found in the pods of each species, which were sent in the uncleaned condition. The samples of *Trifolium repens* showed from 1–4 seeds in each legume. *Trifolium elegans* and *T. montanum* had 1–3 seeds. *Trifolium resupinatum*, *T. rubens*, and *T. suffocatum* yielded 1–2 seeds. The following species, arranged alphabetically, had only one seed in each pod, and this held good for these species from different countries: *agrarium, alexandrinum, alpestre, angustifolium, arvense, badium, bocconi, dubium, elegans, incarnatum, johnstoni, leucanthum, maritimum, medium, minus, montanum, nivale, ochroleucum, pannonicum, pratense, procumbens, reclinatum, repens, repens* var. *macrorhiza, resupinatum, rubens, scabrum, spadiceum, squarrosum, stellatum, striatum, suffocatum.*

Weight of Seeds.

The weight of the seeds was determined in milligrams. Some of the samples were sufficiently large to permit the weighing of one hundred seeds of each. Others contained only a small number of seeds, so that ten was the maximum number the weight of which could be determined. In order that the results might be made comparable, the weights in each case are calculated on the ten basis. Of course, the weighings were more accurate where the larger numbers were used. In some cases fifty seeds and in others twenty-five seeds were weighed. In the accompanying list the weights determined by weighing 100, 50, 25 seeds are placed in ordinary type. The weights originally determined on the ten basis are printed in heavy-faced type. The weights of the seeds of the various species of Trifolium are in milligrams: *agrarium* (2, 5, 3.2, 5), *alexandrinum* (30, 31.5, 32.5, 34), *alpestre* (10, 12, 15.5), *altissimum* (17), *angustifolium* (12, 14), *arvense* (2, 2.5, 4), *aureum* (3), *balansæ* (5.4), *bocconi* (3, 5.5), *campestre* (1.2), *cernuum* (2), *dubium* (**3.3**), *elegans* (4.5, 6.5, 7.5), *filiforme* (4.5, 5, 5.5, 5.6), *glomeratum* (2.5, 3), *hybridum* (5.5, 5.5, 5, 7, 7.5, 8,8) *incarnatum* (30, 32.3, 36, 36.5, 36.5, 37, 42.5), *johnstoni* (5.3), *lupinaster* (17), *maritimum* (15, 17, 25), *medium* (12.5, 12.5, **13**), *minus* (3.3, 5), *montanum* (**5**, **6.5**, 7.5, **10**, 11), *nivale* (5), *pannonicum* (28, 33, 36.5, 37, 40, 41), *patens* (6.5), *perreymondi* (3), *pratense* (13, 13.5, 14, 15, 15, 16, 16, 16.5, 17, 19.5, 20, 22, 25),

pratense perenne (18.5), *pratense* × *medium* = Sutton's cowgrass (21), *pratense* × *pratense* × *medium* = Sutton's giant hybrid cowgrass (19.5), *procumbens* (2.5, 2.9, 5), *reclinatum* (30, 31), *reflexum* (6, 6.5), *repens* (2, 3, 3.5, 3.7, 5.5, 5.5, 7, 7, 7, 7.5, 8), *repens* var. *macrorhiza* (5.5), *repens perenne* (7.5), *repens* var. (4.7), *resupinatum* (8.5, 10, 11), *rubens* (18, 18.5, 19.5, 21, 22.5, 23.5), *scabrum* (1, 2, 4.5), *spadiceum* (5), *spumosum* (2.5), *squarrosum* (15), *striatum* (20, 20.6), *suaveolens* (16.5), *subterraneum* (26, 34, 119.6[1]), *tridentatum* (13.5). If we classify the species according to the weight of their seeds into light-seeded, medium-seeded, heavy-seeded, we would have the following arrangement of them:

Light-seeded Clovers.—agrarium, arvense, aureum, balansæ, bocconi, campestre, cernuum, dubium, elegans, filiforme, glomeratum, hybridum, johnstoni, minus, montanum, nivale, patens, perreymondi, procumbens, reflexum, repens and varieties, *scabrum, spadiceum, spumosum.*

Medium-weight Seeds.—alpestre, altissimum, angustifolium, lupinaster, maritimum, medium, pratense, squarrosum, suaveolens, tridentatum.

Heavy-seeded Clovers.—alexandrinum, incarnatum, pannonicum, reclinatum, rubens, striatum, subterraneum.

The weight of seeds is an important matter to know in buying seeds for farming operations by bulk, and also it can be used in helping to identify doubtful seed samples, as there are general specific differences in the weight of seeds. There is some degree of correlation between the weight of clover seeds and their size. The larger seeds are heavier than the smaller ones. It is probable that the differences in the weight of two samples of the same species of clover are due also to a difference in their age. The older seeds, having lost water in drying, are, of course, lighter than the younger seeds, which have not dried out to the same extent. The variation in weight may be due to the fact that they have been derived from different countries, and, therefore, grew under totally different conditions.

[1] This apparently large discrepancy is due to the fact that the seed size and weight varied more in this species than in any other.

Seedling Clovers.

An examination of the large number of seedlings raised from seeds obtained from the different places mentioned above shows that they may be classified into several groups according to the sizes of the cotyledons. The following details are taken from the natural-sized drawings of all the seedlings raised during their experimental study (Plates I, II, III). The species of Trifolium with large cotyledons are: *alexandrinum, angustifolium, incarnatum, pannonicum, pratense.* Those with middle-sized seed leaves are: *agrarium, alpestre, angustifolium, maritimum, medium, procumbens, ochroleucum, reclinatum, spumosum, striatum, subterraneum.* The clovers which showed small cotyledons on germination are: *arvense, aureum, badium, balansæ, bocconi, campestre, cernuum, elegans, filiforme, glomeratum, johnstoni, lupinaster, minus, montanum, patens, perreymondi, resupinatum, rubens, scabrum, spadiceum, suffocatum.* The clovers with narrow cotyledons are: *agrarium, angustifolium, bocconi, campestre, filiforme, glomeratum, maritimum, patens, reclinatum, repens, scabrum.* Those with broad seed leaves are: *alexandrinum, alpestre, badium, elegans, incarnatum, lupinaster, minus, ochroleucum, pannonicum, pratense, resupinatum, spadiceum, spumosum, stellatum.* The length of the hypocotyl and radicle together varied from 7 mm. in *T. johnstoni*, 8 mm. in *T. scabrum*, to 60 mm. in *T. incarnatum.* All of the radicles developed root hairs in greater or less numbers. The accompanying plates (Plates I–III) give the general form and appearance of the clover seedlings examined.

Morphologic Variations in the Clovers.

It is not intended to make an exhaustive survey of morphologic variations in the clovers, but to briefly describe those which came under the personal observations of the writer.

Double-headed Red Clover.—In July, 1911, at Belmar, N. J., were found two red clover plants in which the heads were double. The twin heads were separated down to the middle and were united by their lower halves. The flowers were of the usual structure and color.

Statistical Study of Red Clover Variations.—The usual statements in the manuals of botany about the size and other characteristics of the organs of the species included in the manual are not based on accurate measurements for statistical purposes. In order to provide such data for the common species of Trifolium, measurements were made of one hundred red clover plants as a beginning.

The length of the petioles above the stipules of one hundred leaves varied from 271 mm., the longest, to 38 mm., the shortest. The length of the middle leaflet of the three leaflets varied from 44 mm., the longest, to 15 mm., the shortest. The widest middle leaflet was 29 mm., and the narrowest 12 mm. Similarly measurements were made of the right and left leaflets of the trio with the following results:

Longest Left Leaflet	42 mm.
Shortest Left Leaflet	15 mm.
Widest Left Leaflet	26 mm.
Narrowest Left Leaflet	12 mm.
Longest Right Leaflet	43 mm.
Shortest Right Leaflet	13 mm.
Widest Right Leaflet	29 mm.
Narrowest Right Leaflet	10 mm.

Out of 100 leaves, 78 had leaflets with retuse apices and 22 had leaflets with obtuse apices. The three leaflets of our common field red clover are marked usually (89 out of 100) with U-shape, or V-shaped, whitish blotches, as if one had placed a dab of white paint on the leaflets with the fleshy part of the thumb. In some cases these markings are pale, in other cases prominent. Eleven plants in one hundred were found entirely without the thumb-mark spots. Sometimes a red clover plant has been found in which the white color runs along the veins in streaks toward the midribs of the leaflets. This arrangement of color may be due the action of an enzyme in producing the variegation, or to the presence of air beneath the surface in the V-shaped area. A little boy, Roger M. Hinckley, aged 11, writes to St. Nicholas (1909) from Greenfield, Mass., about a crinkly-leaved white clover with leaflets panduriform. A figure is given of this variation, probably induced by disease.

Four red clover plants had leaves with glabrous (smooth) petioles and 96 red clover plants of my collection had petioles that were hirsute. Five clover plants in one hundred had blotched leaves and 95 were unblotched. This descriptive presentation of the statistical study of one hundred red clover plants has been adopted instead of the tabular form to save the cost of the printing of the accumulated data in the form of a table.

Clover Habitats and Growth Forms.

The various habitats described with reference to the species of clover found growing in them were taken from the descriptive labels of the herbarium sheets found in the herbarium of the Botanical Department, University of Pennsylvania, the Academy of Natural Sciences of Philadelphia, the U. S. National Herbarium, Washington, D. C., the New York Botanical Gardens, and Kew Gardens, London, England. The habitats are arranged beginning with the hydrophytic ones and ending with the driest, most xerophytic ones.

Wet Places.—*Trifolium amplectens* (California), *anodon* (California), *arizonicum* (California), *barbigerum* (California), *bolanderi* (California), *cyathiferum* (California, Oregon), *harneyensis* (Oregon), *howellii* (Oregon), *megacephalum* (Oregon), *melananthum* (California), *microcephalum* (Washington), *oreganum* (Washington), *pauciflorum* (Washington), *pedunculatum* (Oregon), *rydbergii* (Montana), *scabrellum* (California), *splendens* (California), *truncatum* (California), *variegatum* (California).

Salt Marsh Edge.—*Trifolium flavulum* (California).

Gravelly River Bar.—*Trifolium heterodon* (Washington).

Meadows.—*Trifolium andrewsii* (California), *beckwithii* (Oregon), *harneyensis* (Oregon), *involucratum* (Washington), *longipes* (California), *macræi* (California), *monanthum* (California), *plumosum* (Idaho), *polydon* (California), *reflexum* (Arkansas), *rusbyi* (California), *spinulosum* (California), *tridentatum* (California), *triste* (California), *wormskioldii* (California).

Pampas.—*Trifolium matthewsii* (Argentina).

Prairies.—*Trifolium altissimum* (Washington), *amphianthum* (Texas), *bejariense* (Texas), *carolinianum* (Texas), *douglasii* (Washington), *pauciflorum* (California).

Open Gravelly Mountain Slopes.—*Trifolium virginicum* (West Virginia).

Rocky Woods.—*Trifolium aureum* (Maryland).

Calcareous Bluffs.—*Trifolium mexicanum* (Mexico).

Pine Woods.—*Trifolium barbigerum* (California), *microcephalum* (California), *microdon* (California), *splendens* (California), *stenophyllum* (California), *subcaulescens* (Colorado), *trichocalyx* (California), *variegatum* (California), *wormskioldii* (California).

Big Tree Groves.—*Trifolium breweri* (California).

Alpine.—*Trifolium alpinum* (Switzerland, Tyrol), *amabile* (Bolivia, Mexico), *attenuatum* (Colorado), *badium* (Pyrenees, France), *bracteolatum* (Colorado), *brandegei* (Colorado), *burchellianum* (Africa), *caudatum* (Phrygia), *chilense* (Bolivia), *calocephalum* (Africa), *dasyphyllum* (Colorado), *gemniflorum* (Colorado), *haydeni* (Montana), *involucratum* (Mexico), *lividum* (Wyoming), *montanense* (Montana), *nanum* (Colorado), *noricum* (Carniolia), *pallescens* (Switzerland), *parryi* (Colorado), *petrophilum* (California), *polystachyum* (Africa), *salictorum* (Nevada), *saxatile* (Switzerland), *schiedeanum* (Mexico), *semipilosum* (Africa), *subrotundum* (Africa), *stenolobium* (Colorado), *thalii* (Pyrenees).

The growth forms given on the labels of the herbarium sheets and also noted when the clovers were studied are:

Dwarf Alpine Plants with Large Tap Root.—*Trifolium amabile* (Bolivia), *andinum* (Wyoming), *anemophilum* (Wyoming), *barbigerum* (California), *bolanderi* (California), *bracteolatum* (Colorado), *chilense* (Bolivia), *dasyphyllum* (Colorado), *lividum* (Wyoming), *nanum* (Colorado), *pallescens* (Switzerland), *salictorum* (Nevada), *scariosum* (Montana, Wyoming).

Dwarf Rosette.—*Trifolium andersonii* (Nevada), *bolanderi* (California), *gymnocarpum* (Wyoming), *virginicum* (West Virginia).

Open Woodlands and Prairies.—*Trifolium stoloniferum* (United States).

Grass Steppes.—*Trifolium acaule* (Africa), *simense* (Africa).

Mesas.—*Trifolium aciculare* (California).

Sand Beaches.—*Trifolium alexandrinum* (Syria), *fucatum* (California), *heterodon* (Washington), *macræi* (California), *maritimum*

(England), *repens* (New Jersey), *tomentosum* (Island Porto Santa), *wormskioldii* (California).

Desert.—Trifolium gracilentum (California).

Pedregal.—Trifolium willdenovii (Mexico).

Prostrate Mats.—Trifolium gracilentum (California), *melananthum* (California), *resupinatum* (Madeira), *variegatum* (California).

Prostrate Xerophytes.—Trifolium depauperatum (California), *piluliferum* (Palestine).

GENERAL GEOGRAPHICAL DISTRIBUTION.

One fact which impressed the writer in the study of the clovers (Trifolium) at the Herbarium of Kew Gardens, London, England, in July, 1907, was the large number of species found in California, Oregon, Washington (Pacific North America), and in Palestine and Syria in the near east. In both cases the countries rich in clovers face western oceans, Palestine and Syria facing the Mediterranean Sea, and California, Oregon, and Washington the Pacific Ocean. "Orient" in the enumeration given below probably means the near east, and if the six North African species are deducted, as represented in the Kew Herbarium, there are 85 species from the orient. There are 14 species of clovers given in the second edition of Britton and Brown's "Illustrated Flora," and in the second edition of Small's "Flora of Southeastern United States" 3 species not given for the northern states, making 17 species in all. Seventeen species deducted from the number of North American species (86) in the Kew Herbarium gives 69 species as the number in Western North America, supporting in a general way the statement above, as to distribution in countries facing western oceans. The following numbers are derived from a list of clovers made while at Kew, and, although not complete, they are an indication in an approximate way of the numbers of species of Trifolium in different countries of the world: North African and Orient (91); North African (6); Northern Asia (7); China and Japan (3); India, Malaya (3); India (2); Australia, New Zealand (4); Tropical Africa (20); South Africa (14); North America (86); Central America (9); East Tropical South America

(8); West Tropical South America (1); Temperate South America (12).

Sleep Movements of Clover Leaves.

Pfeffer in his "Untersuchungen über die Entstehung der Schlafbewegungen der Blattorgane," Leipzig, 1907, does not undertake to investigate the clovers, although they provide more available material than *Albizzia lophantha*, *Mimosa Spegazzini*, *Phaseolus*, *Lourea vespertilionis*, *Mimosa pudica*, *Impatiens parviflora*, and *Siegesbeckia orientalis* investigated experimentally by him.

The gross morphologic movements are of interest (Plates D, E). The following species of clovers grown experimentally by the writer move their terminal leaflets into a flat vertical position in their nyctitropic movements: *Trifolium alpestre, arvense, dubium, elegans, filiforme, glomeratum, incarnatum, maritimum, minus, pannonicum, pratense* (mammoth), *pratense* × *pratense* × *medium, reflexum, resupinatum, rubens, spinosum.*

A few clover leaflets move so that the terminal leaflet is erect, but folded somewhat inwardly along the midrib. Such are *hybridum, ochroleucum, pratense, pratense* (old), and *subterraneum.* In these two cases the lateral leaflets move upward and inward so that their surfaces are applied together, the leaflets standing vertically as the figures clearly show. The following species of Trifolium show the terminal leaflets bent forward out of the vertical through an angle of 10° to 30° from the vertical: *alexandrinum, angustifolium, cernuum, elegans, hybridum, incarnatum, johnstoni, leucanthum, medium, perreymondi, reclinatum, striatum, tridentatum.*

In *Trifolium cernuum, elegans, medium, repens, perenne,* the terminal leaflets are folded along their midribs, so that their leaflets overlap the two lateral leaflets which stand vertically and lie parallel.

In *T. montanum, repens* (old), *repens perenne, scabrum,* the terminal leaflets are nearly horizontal, forming almost an angle of 90° with the vertical. The positions of the leaflets in *T. agrarium, aureum, badium, bocconi, campestre, patens, procumbens, stellatum* are different from the other species and are not suggestive of sleep movements. Whether the nyctitropic positions assumed in these clovers are due to experimentally unhealthy plants, future study of

them alone will reveal. The terminal leaflets of *Trifolium lupinaster* and *reclinatum* bend backward out of the vertical instead of forward. The accompanying 62 drawings will demonstrate better than words the actual position of the leaflets in the various clovers during nyctitropism (Plates IV, V).

EXPERIMENTAL CURVES OF NYCTITROPIC CLOVER LEAVES.

The elaborate and rather costly piece of apparatus used by Pfeffer in his work, described and figured in Chapter II. of his work, was replaced by a simple home-made piece of apparatus (kymograph) consisting of a tin tomato-can mounted on an alarm clock with works altered to suit the conditions of the experiments. The tomato-can cylinder with registering paper was made to revolve once in two days instead of once every twelve hours by changing the motion of the bar turning the hour hand of the clock. The recording lever was poised on a fulcrum fastened to a cork pushed into the mouth of a test tube, which was then attached to the arm of a tripod, on the pan of which the potted clover plant was placed. A fine thread was attached to the clover leaflets by a thick solution of shellac and to the short arm of the registering lever. The long arm of this lever being twice as long as the short arm, the movements were thus magnified twice. The downward movements of the leaflets were transmitted into upward movements of the far end of the lever which traced the curve of movement on the glazed paper, which was covered with a coating of carbon by holding over a yellow gas flame after being placed on the cylinder. The curves represented in the accompanying figures were obtained in this way and were made permanent records by passing the blackened paper through a solution of shellac in 95 per cent. alcohol.

Fifty-five records were taken during the course of the experiments with the kymograph. The species of Trifolium, the movements of the leaflets of which were traced by curves, were as follows: 1, *resupinatum;* 2, *subterraneum;* 3, *incarnatum;* 4, *pannonicum;* 5, *elegans;* 6, *reclinatum;* 7, *alpestre;* 8, *spumosum;* 9, *leucanthum;* 10, *tridentatum;* 11, *pannonicum;* 12, *lupinaster;* 13, *pratense;* 14, *alpestre;* 15, *hybridum;* 16, *medium;* 17, *elegans;* 18, *balansæ;* 19,

stellatum; 20, *filiforme*; 21, *rubens*; 22, *dubium*; 23, *ochroleucum*; 24, *minus*; 25, *angustifolium*; 26, *elegans*; 28, *alexandrinum*; 29, *incarnatum*; 30, *striatum*; 31, *reclinatum*; 32, *patens*; 33, *maritimum*; 34, *rubens*; 35, *campestre*; 36, *maritimum*; 37, *cernuum*; 38, *bocconi*; 39, *hybridum* (old); 40, *agrarium* (mature); 41, *glomeratum*; 42, *incarnatum*; 43, *pratense* mammoth; 44, *pratense* (old); 45, *repens* (old); 46, *repens* var. *macrorhiza*; 47, *johnstoni*; 48, *pannonicum*; 49, *ochroleucum*; 50, *reclinatum*; 51, *procumbens*; 54, *reflexum*; 55, *pratense* × *pratense* × *medium*; 56, *badium*; 58, *perreymondi*; 59, *arvense*; 60, *aureum*; 61, *scabrum*; 62, *repens perenne*; 63, *agrarium* (young).[2] Not all of the records are good. Some of them are incomplete owing to the stoppage of the driving clock, or to the swinging of the level away from the recording cylinder owing to the disturbance of the lever stand. Occasionally the thread would break loose from the shellac fastening. As a result of these difficulties, some of the records are only part curves, and, therefore, can not be considered in the subsequent discussion. Only those records are discussed and illustrated which are fairly complete. It was found impracticable after the taking of the curve of the first species (*T. resupinatum*) to make the curve of temperature on the same record sheet as the curves of the leaflet movements. No attempt, therefore, was made to take the temperature in subsequent records. The relative humidity was not recorded in connection with the experimental study of the plants, nor was the relative amount of sunlight and cloudiness registered. Some changes in the curves of the night period suggest that it might have been advisable to have data on the times and intensity of the moonlight. The curves which are discussed in the following pages may be criticized, as approximations, because no record of temperature, humidity, sunlight, and cloudiness were kept in connection with them. This criticism is undoubtedly well founded, but the several complete curves are given, because they are suggestive of the character of the clover-leaf movements and may create enough interest for some one else to continue the careful investigation of such leaf movements as related to all of the influential environmental factors.

[2] The numbers here given apply to the drawings of seedlings (Plates I–III) and leaf movements (Plates IV–V).

Plant 1, Trifolium resupinatum (June 1 to June 4, 1908).—The terminal leaflet was attached to the string at 8 A.M., June 1. The curve was fairly level until the middle of the afternoon, when it began to rise with the approach of darkness. The steepest part of the curve was reached during the night, gradually sloping downward until morning. Some irregularities in the curve are noticeable on June 2. These may be due to cloudiness, for other records show that an overcast, or heavily clouded sky, causes the rise of the curve. This is especially noticeable on June 3, for there is a sharp rise after the depression of the morning. The sharp bumps in the curve of *Trifolium resupinatum* indicate the nyctitropic movements of the leaflets. The curve of Plant 2, *T. subterraneum*, demonstrates the same response of the terminal leaflets. There was a steady rise on June 3 in the afternoon, followed by a steep depression until nightfall, when the up curvature during night becomes a marked elevation, followed by a gradual depression until 9 A.M. on the following day, June 4.

Plant 9, Trifolium leucanthum (Plate V).—Experiments with this plant were started at 8 A.M. on June 2. The curve shows an upsweep in the forenoon, followed by a downward curvature in the middle of the day. At 3.20 P.M. we note a steep elevation, then a drop with a steep rise to 6.15 P.M., succeeded by a sharp drop of the curve and its reëlevation during the night. The curve taken on June 3 shows minor oscillations of the lever during the day and a sharp rise at 6 P.M. for the night. With the approach of day the curve drops uniformly until 8 P.M., when it rises slightly, dropping again at 9 A.M.

Plant 14, Trifolium alpestre.—This plant gave a curve which started at 11.20 A.M., June 4. It shows a uniform rise to the middle of the afternoon, then a drop and a sharp rise at nightfall. On the morning of June 5 there is a uniform drop of the curve with some oscillations until 3 P.M., when it begins to rise.

Plant 17, Trifolium elegans (Plate V).—The curve of leaf movements in this clover shows a gradual rise from the noon hour to late afternoon, with a sharp upward curvature at night, followed by a

depression the following day, June 5, and a sharp rise to the night position at 6 P.M.

The morning of June 6 showed a gradual downward movement of the curve until 9 A.M.

Plant 39, Trifolium hybridum.—A mature plant in flower gave a curve with a sharp valley and peaks. Starting at 12 M., June 10, it shows a sharp angular drop to 3 P.M., then a sharp rise and as sharp a depression during the late afternoon. After nightfall there was a precipitous rise during the night, then a sharp drop and a rise until about 10 A.M. on June 11. The curve then took a drop to noon, when it began to rain, and a sharp rise to the late afternoon, when it probably cleared. At 6 P.M. it made a steep up curvature during the period of night.

Plant 40, Trifolium agrarium (an old plant).—A lateral leaflet was tried, so that its curve does not correspond in general with the curves taken for the terminal leaflets. Starting at 11 A.M., June 12, we find the elevation of the curve is followed by a steep depression until 6 P.M., when the curve remains nearly horizontal until the next morning, when it rises rapidly to 9 A.M., followed by a depression until 3 P.M., when there is a rise to 6 P.M., and then a gradual fall.

Plant 42, Trifolium incarnatum (Plate V).—The curve of the crimson clover is fairly constant with an almost level depression during the day of June 10, a rounded elevation during the night, a depression with a sharp hump in the early morning of June 11 before 9 A.M., followed by a depression and a small rise after 12 M., when it began to rain. At 6 P.M. the upward movement of the curve for the night period is observable.

Plant 45, Trifolium repens.—An old mature plant in flower was studied as to the movements of the terminal and lateral leaflets. The curve for the terminal leaflet rose from 11 A.M., June 12, during the afternoon with a sudden downward movement to 6 P.M. The night period was characterized by a steep upward curvature followed by a drop during the small hours of the night, and a sharp rise before 9 A.M. on June 13. It is such a sharp rise which leads one to suspect that moisture may be a controlling factor. A rise of the curve to 3 P.M. is observable, and then a gradual downward curva-

ture to 6 P.M., when an upward movement is seen. The curve of the lateral leaflet of No. 45 is generally up during day and down during the night, with a steep downward gradient as the hour of sunrise is approached, with a sharp upward curvature in the daylight hours of the morning to 9 A.M. on June 11, then with a rainy morning, a downward movement is seen with an upward turn in the early afternoon.

Plant 48, Trifolium pannonicum (Plate V).—The middle leaflet of the compound leaf of this plant was connected with the kymograph at 11 A.M., June 12, with an oscillating movement of the curve downward to nearly 6 P.M., when the upward curvature during the night is clearly traceable. Daylight of June 13 shows a valley-like depression with sharp irregularities and a strong upward turn at nightfall. The same valley-like trough of the curve appeared for the daylight period of June 14, with a sharp rise on the afternoon of that day. Such sharp oscillations are inexplicable unless they are due to variations of temperature or humidity of the air. An inspection of the several curves in the accompanying plate will give a more adequate idea of the movements of the long arm of the recording lever. The above description will serve to draw attention to the more important features of the curves.

The writer desires to acknowledge the help given by Professor E. B. Ulrich in the arrangement of the details of the kymograph used in the experimental work. The kymographs used in the study of the clovers are essentially like the ones used by Professor Ulrich in his study of the "Leaf Movements in the Family Oxalidaceæ," published as a thesis[3] in part fulfillment of the requirements for the degree of Doctor of Philosophy at the University of Pennsylvania, received June, 1910.

EXPLANATION OF FIGURES OF THE SIX PLATES.

(PLATES I–VI.)

1. *resupinatum.*
2. *subterraneum.*
3. *incarnatum.*
4. *pannonicum.*
5. *elegans.*
6. *reclinatum.*
7. *alpestre.*
8. *spumosum.*

[3] Contributions from the Botanical Laboratory, University of Pennsylvania, III. : 211–242.

9. *leucanthum.*
10. *tridentatum.*
11. *pannonicum.*
12. *lupinaster.*
13. *pratense.*
14. *alpestre.*
15. *hybridum.*
16. *medium.*
17. *elegans.*
18. *balansæ.*
19. *stellatum.*
20. *filiforme.*
21. *rubens.*
22. *dubium.*
23. *ochroleucum.*
24. *minus.*
25. *angustifolium.*
26. *elegans.*
27. *balansæ.*
28. *alexandrinum.*
29. *incarnatum.*
30. *striatum.*
31. *reclinatum.*
32. *patens.*
33. *maritimum.*
34. *rubens.*
35. *campestre.*
36. *maritimum.*
37. *cernuum.*
38. *bocconi.*
39. *hybridum* (old mature plant).
40. *agrarium* (old mature plant).
41. *glomeratum.*
42. *incarnatum.*
43. *pratense* (mammoth).
44. *pratense* (old mature plant).
45. *repens* (old mature plant).
46. *repens* var. *macrorhiza.*
47. *johnstoni.*
48. *pannonicum.*
49. *ochroleucum.*
50. *reclinatum.*
51. *procumbens.*
52. *rubens.*
53. *montanum.*
54. *reflexum.*
55. *pratense* × *pratense* × *medium.*
56. *badium.*
57. *pratense* (mammoth).
58. *perreymondi.*
59. *arvense.*
60. *aureum.*
61. *scabrum.*
62. *repens perenne.*
63. *agrarium* (young).

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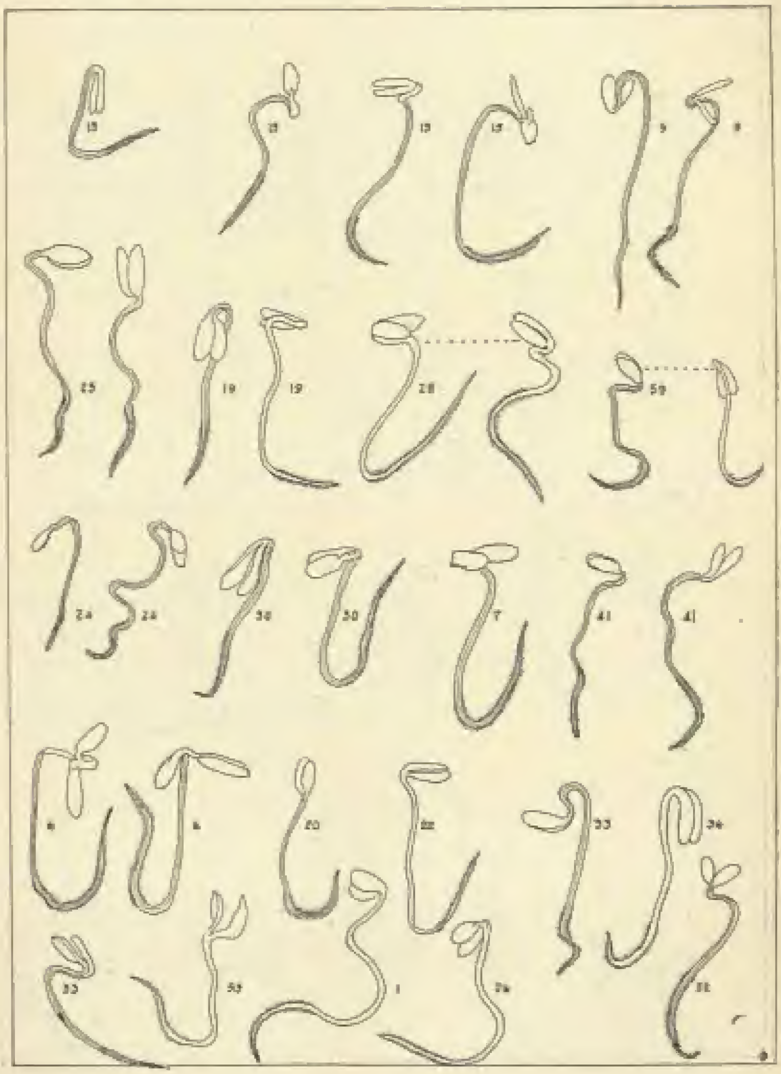

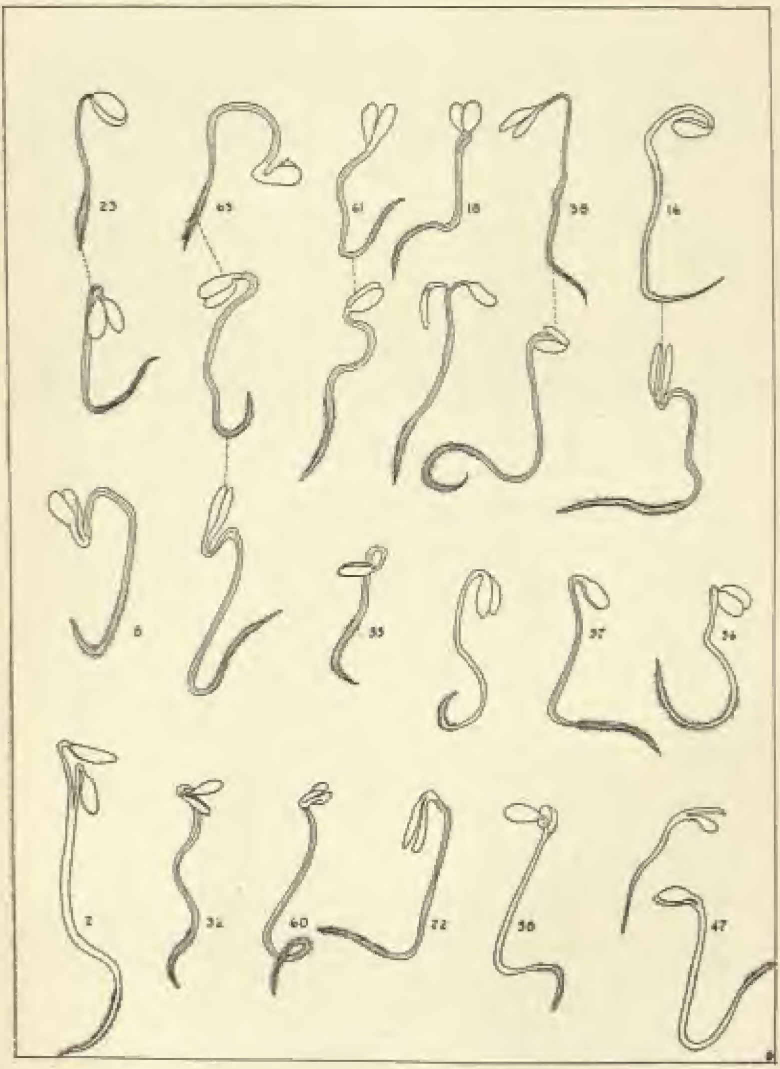
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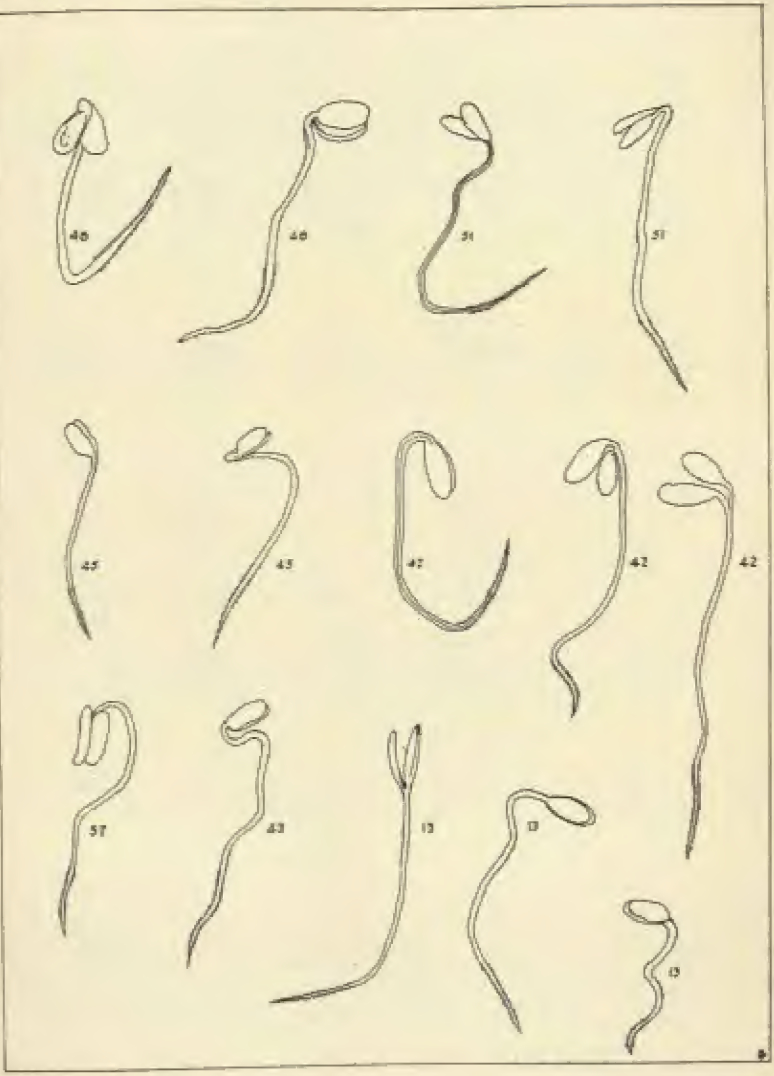
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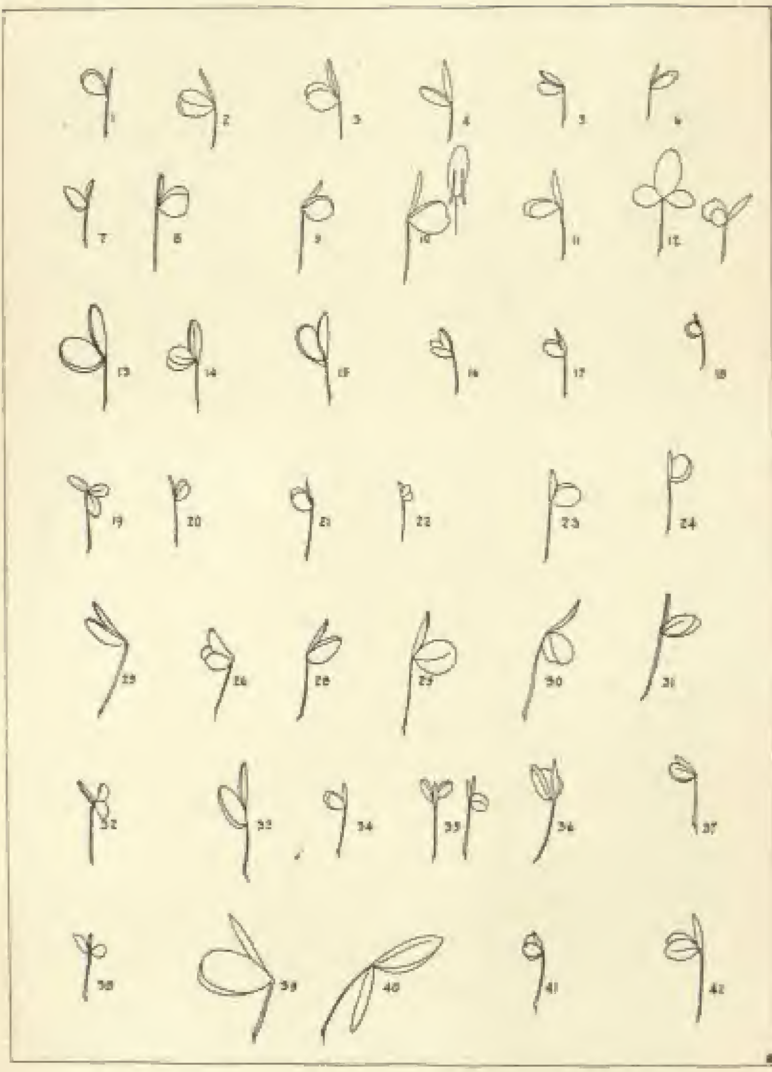

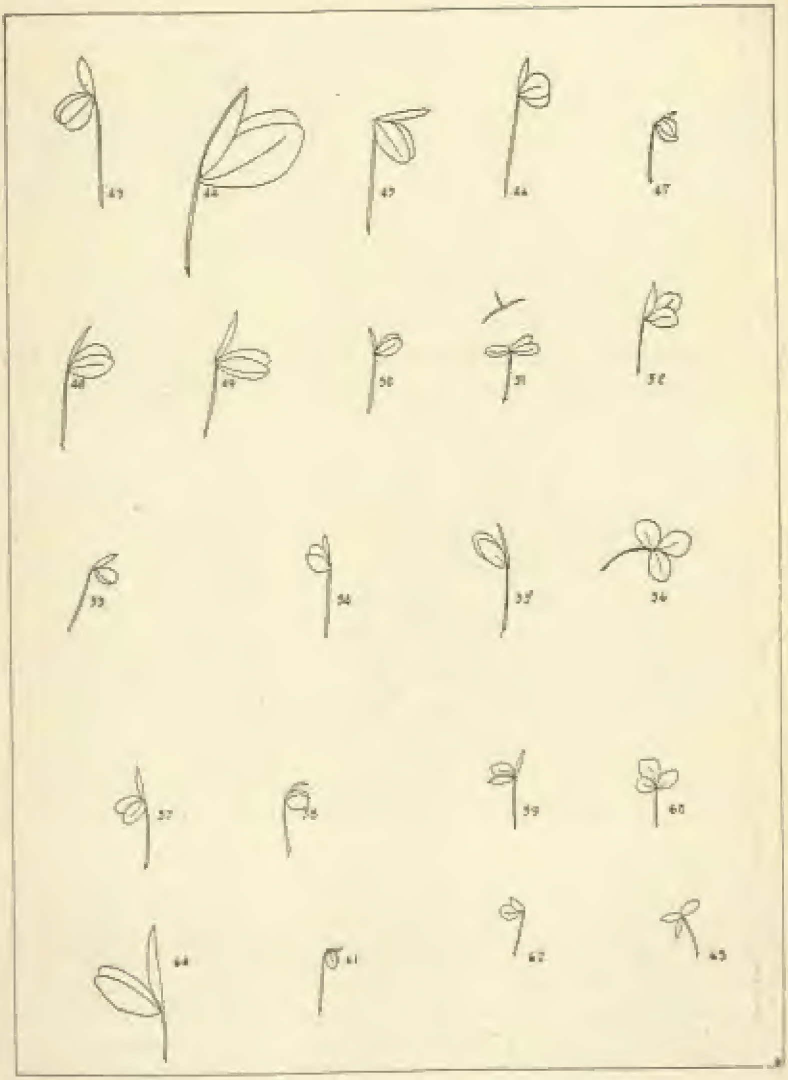

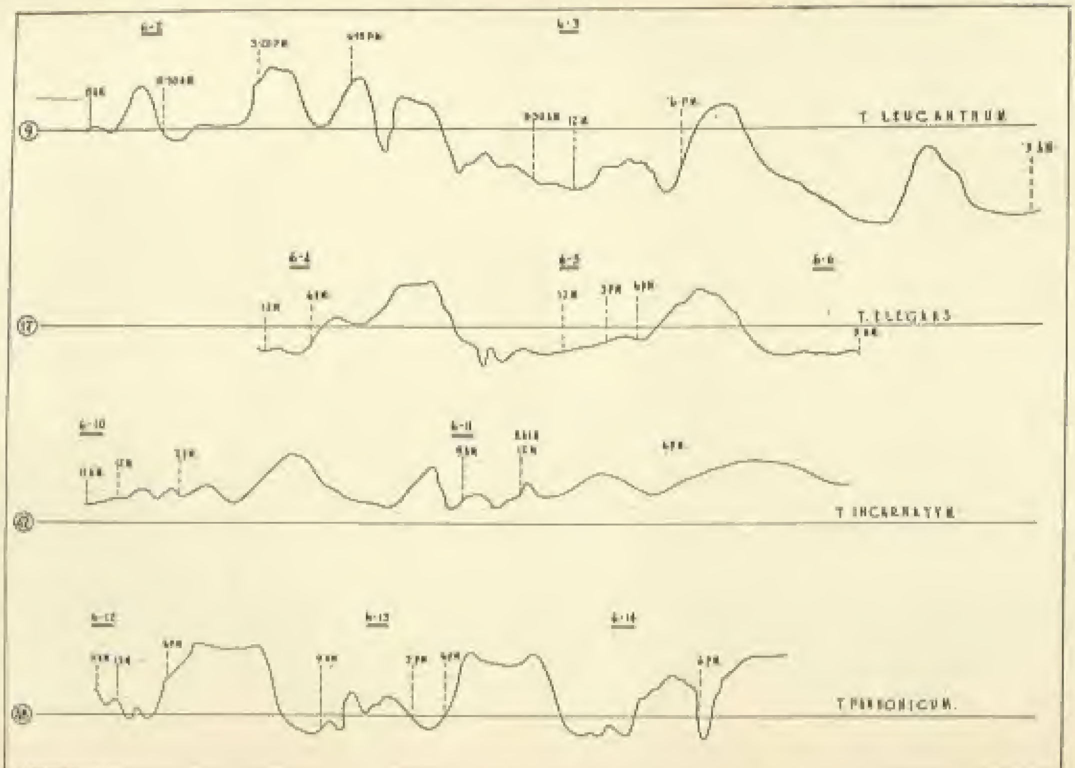
T. LEUCANTHUM
T. ELEGANS
T. INCARNATUM

THE FAEROE LANGUAGE.

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The Faeroe Islands (Sheep Islands), consisting of some twenty-one isles, situated nearly half way between Iceland and the Shetlands at the intersection of 7° E. and 62° N., have an area of approximately 511 square miles and a population of over 18,000. The inhabitants are all of Norwegian descent, their settlement dating from about the beginning of the ninth century, when the first emigrant from Norway, Grim Kamban, came to the Faeroes to escape the exactions of the militant Norwegian king, Harald Haarfager. According to tradition, there was a colony of Irish and Scottish monks, probably Culdees,[1] on Suderoy,[2] when the first Norsemen arrived. These ascetics were driven out almost immediately and the settlement became exclusively Scandinavian. The islands were transferred from Norway to Denmark in 1386 and Denmark's possession was confirmed by the Peace of Kiel in 1815. The Faeroes are now reckoned as an integral part of Denmark, rating as an *Amt* (governmental district), with, however, a local parliament (*Lagting*) consisting of an *Amtmand* (district governor) and nineteen other members. This body elects one representative to the Upper House (*Landting*) of the Danish Parliament (*Rigsdag*), while the Faeroe electors choose by direct vote one representative to the Danish Lower House (*Folketing*). The capital town of the Faeroes is Thorshavn.

Of late, there has arisen upon the Faeroes a very energetic linguistic movement aiming to elevate the local idiom to the rank of a

[1] The Irish name is *Céle Dé* "Comrade of God" and seems to have been used to indicate early Gaelic anchorites whose chief establishments were in Scotland during the 12th century. They were undoubtedly in Iceland before the early Norse occupation there in 1000 A.D., whence they fled, leaving no trace save a few place-names (cf. W. Reeves, The Culdees of the British Islands (Dublin, 1864); W. F. Skene, Celtic Scotland (1876–1880), Vol. II.; W. Beveridge, Makers of the Scottish Church, 1908. See also J. Jamieson, Historical Account of the Ancient Culdees, 1811).

[2] The southern island of the Faeroe group.

by the recent declaration of the independence of Iceland (1918). The Faeroe movement, however, is not a political one, as there are few if any persons in the islands who desire a "national" existence apart from Denmark. The activity should rather be compared with the Welsh language movement in Wales and Monmouthshire. In the Faeroes the tendency is to crystallize the language by taking the most characteristic elements of the two most important dialectic groups—*i.e.*, the northern and the southern—and consolidating them into a "language," which shall form a standard for the whole group. It is perhaps unfortunate for this purpose that the orthography of the older literary Faeroese Norse, whose chief monument was the Faereyinga Saga (translated by F. Yorke Powell, London, 1896), is essentially that of Icelandic, while the actual modern Faeroese pronunciation differs so widely from that of modern Iceland that Faerings and Icelanders are today mutually unintelligible. In spite of this fact, however, conservative Faering scholars insist upon retaining the earlier impracticable spelling. A new orthography was promulgated in 1895 by the *Föringafelag* (Faeroe Society), but this system is far from satisfactory from a phonetic point of view. At the present day, a new school has arisen which is inclined to spell almost entirely in accordance with the actual utterance of the people, but their system is not followed, for example, by the local newspaper *Thingakrossur*.[4] A striking parallel to this state of affairs may be seen in the stereotyped archaic spelling of the modern Gaelic, observed both in Ireland and Scotland, which quite ignores the real modern phonetics of the spoken dialects.

It is quite apparent that the Faeroese phonetics have suffered much from contact with Danish, which is phonetically very degenerate, even when compared with the Norwegian-Danish pronunciation of the same language. The Norwegians utter almost every consonant clearly and have not marred their idiom with the Danish glottal catch,

[3] See bibliography in this article.

[4] A word indicating the cross which in former times was sent from house to house, to summon the men to the *Thing* (law-making assembly). This paper is rather radical, but very mildly so from the present European point of view. The conservative organ is *Dimmalaetting* 'the lifting of the darkness.'

save in one south Norse dialectic group. This sound, which consti-
culture language,[4] a tendency which has undoubtedly been stimulated
tutes such a difficulty for strangers who wish to acquire the genuine Danish utterance, has not penetrated the Faeroe language to any extent,[5] although this vernacular has lost much of its original strength through the disappearance of consonants. Many Danisms have crept into the vernacular, but the purist school is now trying to eradicate and replace them by older expressions. Faeroese, like modern Icelandic, has no distinctive tones, which play so prominent a part both in Norwegian and Swedish.

I am indebted for almost all the material embodied in the following brief sketch to Miss M. E. Mikkelsen, a Faeroese lady now resident in Copenhagen, who has been kind enough to give me the phonetics of her native idiom and to sing for me several of the characteristic ancient songs, one of which has been reproduced at the end of this article. There is a small colony of Faerings in Denmark proper, chiefly students at the Copenhagen University, who keep up their island idiom and character as far as possible.

The following extracts in both the accepted spelling and its phonetic equivalent (with translation) will serve to illustrate the chief differences of pronunciation existing in the modern use. The rules for pronouncing the current orthography may be tabulated thus:

Vowels: *a*; very flat as *ae* = *a* in 'hat'; *á* = *oh*, when short; when long = *oh-a*, a harsh diphthong; *e* = *e* when short; when long = *ae* = *a* in 'hat,' a sound well represented by *ä*; *i* and y as *i* in 'pin'; *í* and *ý* as *ui* (*ooi*), in some dialects as *üi*; *o* as *o* in 'smock,' sometimes as *ö*; *ó* as Eng. long *o*, sometimes contracted to *e* or even *ö*; *u* as Eng. *oo* in 'foot'; *ú* as Eng. *u* in 'mute'; *ae* as flat *a* in 'hat' in short syllables, but as *äa* in long syllables, something like the Canadian diphthongal pronunciation of 'man' (*mäan*); *ö* as Germ. *ö*, open in short syllables and closed in long syllables.

Consonants: *dj* and *gj* as Eng. *j*;[6] *dh*[7] is never pronounced as *th* in 'this,' as it occurs in mod. Icelandic, but is uttered as *y* when

[5] It seems to appear in such pronunciations as *fö'lk* 'people.'

[6] In some dialects as palatalized *dy*.

[7] The combination *dh* is used throughout this paper, owing to the difficulty of obtaining the Icelandic character representing this sound.

it precedes *i*, except when *u*, *ú* and *ó* precede the *dh*, in which case *dh* is pronounced like *v*, as *godhir*=*govir*, but *lidhur*=*luiyur*. *Dh* is always *v* before *u*, unless *i*, *í*, *y*, *ý*, *ei*, *ey*, *oy* precede *dh*, as *madhur*=*mavur* 'man'; *mudhur*=*muvur* 'mother,' with which cf. Cockney *muvver*. *Dh* is silent in combinations when it occurs between two vowels other than those mentioned and when it is in combination with another consonant or in the *Auslaut*. Note that the hard *th* sound as in 'think' has disappeared in F., where it is now replaced by *t*, as *tadh* 'that,' pr. *tää*=Icel.[8] *thadh*.

G is hard except before *e*, *i*, *y* and *ey*, when it=Eng. *j*. *G* is silent between two vowels changing to *y* or *v*, as *siga*=*siya*; *dagur*=*davur*. *Gj* always=Eng. *j*.

Hv=*kv*, with which cf. Icel. *hv*=*khv*.

K is hard except before *e*, *i*, *y*, *ey*, when it=Eng. *ch*. *Kj* always =Eng. *ch*.

L is very soft, as in Russian soft *l*. *Ll*=*ddl*. *Ng* is always as in 'finger,' never as in 'singing.' *Nn* after a diphthong sounds like *ddn*, otherwise as *nn*. *R* is always trilled except before *t* (*-rt*), when it is pronounced *-rst*. Note that in Mod. Icel. final *-r* is always *-rs*.[9] *Rn*=*ddn*, but in some words=*rn*, which must be learned by practice. *Sj* and *stj*=Eng. *sh*. Single *s* is always hard as in 'this.' *Tj*=Eng. *ch*.

DIPHTHONGS: *ei*=*ai* (*i* in 'hide'), never as in Icel. *ey* (*ay* in 'may'). *Ey*=*ei* (Eng. *ay* in 'may'); *oy*=*oi* (*oy* in 'boy').

PHONETIC SPECIMENS.

I. Faeroe "National" Song.[10]

Eg oyggjar veit sum hava fjöll *E oydshar vait sum haeava fyödl*	I know isles which have moun-
Og gröna lidh. *O gröena lui*	tains and green hill-sides.
Og taktar eru taer vidh mjöll *O taktar erre taear vi myödl*	And are covered with fine snow

[8] In this article "Icelandic" always indicates the modern language.

[9] This *rs* is exactly the same sound as in the Osmanli Turkish final *r*, and closely resembles the Czech *ř* (= *rz*) pronounced together.

[10] *Songbók*, p. 2 (see Bibliography).

Um vetrartidh *Um vetrartui.*	in winter time.
Og áir renna vakrar har *O oayir raenna vaeakrar haear*	And streams flow beauteous
Og fossa nogv. *O fohssa naegv* (or *nogv*).	And water-falls enough.
Taer vilja allar skunda saer *Taear vilya adlar skunda saear* I bláan sjogv. *Ui bloan shaegv* (or *shogv*).	These wish all to hasten into the blue sea.
Gud signi mitt födhiland Föroyar *Gud signeh muit föeycland Föryar*	God bless my fatherland the Faeroes.

II. Folktale in Suderoy Dialect.[11]

Eina ferdh fór Pálin undir Hamri i Sunnbö til Víkar at taka sjey
Aina fer fohr Polin ondir Hamri ui Sumbö til Vuikar at taka she
One time went Paul from under H. in Sumbö to Víkar to get for himself

gimbralomb, sum hann átti har. Hann hevdhi fingidh seks, sum hann setti
dshimbralomb, sum han otte haear. Hann heyi finggi seks, sum hann sette
a ewelamb, which he owned there. He had got six, which he put

i stödhukrónna, og seint var á degi tá idh hann fór ettir ti sjeynda.
ui stöukrohna, o saint vaear oa deyi toa ui hann fohr ettir ti sheynda.
in the fold, and it was late in the day before he went back after the seventh.

Ikki fekk hann tadh firr enn stjödna var uppkomin. Til alla vanlukku
Itshi fekk hann tää firr enn shödna vaear upkomin. Til adla vanlo'ku
He did not get this one before the stars were up. Most unhappily

gáadhi hann ikki um[12] *firr enn hann var kominn vael á veg at tadh var*
goayi han itshi um firr enn hann vaear komin vel oa ve at tää vaear
he did not discover before he had come well on his way that that one was

gimburlamb hann var faridh vidh. Tey gomlu hövdu ta pátrugv at var tadh

[11] *Faeröske Folkesogn*, pp. 49–50 (see Bibliography). The phonetics are as given by Miss Mikkelsen and the translation is by myself.

[12] Lit. 'go about a thing' = 'discover it.'

dshimburlamb hann vaear fari vi. Tey gomlu höddu ta poatrukv at vaear tä
a ewelamb he had started with. The ancients had the superstition that were it

seint á degi og fleiri lomb ettir at bera, so átti vedhurlambidh at vera
saint oa deyi o flairi lomb ettir at be-era, so otte vevurlambi at ve-era
late in the day and many lambs were to be carried, a ramlamb ought to be

tikidh,[13] *og gimburlambidh at vera ettir, ti í vedhurlambsstiklinum sat*
titshi, o dshimburlambi at ve-era ettir, tui ui vevurlambsstiklinun saeat
taken, and the ewelamb to be left behind, because taking a ramlamb meant

nakadh idh dugdi imóti gandi, og so kundi huldufolk ikki fáa vald á einum
naka ui dudde imo-uti gandi, o so kunde huldufö'lk itshi foa vald oa ainun
something which worked against witchcraft and so the fairies could not get power over one

(tey gomlu boru tí ofta vedhurlambsstikl uppi á saer). Honum vardh[14]
(tey gomlu boru tui ofta vevurlambsstikl uppe oa saear). Honun vaear
(the ancients often undertook this carrying ramlambs). He became

illa vidh, tá idh hann varnadhist um hetta, men ovseint var at
idla vi, toa ui hann vadnayist um hetta, men oasaint vaear at
angry, when he discovered about this, but it was too late to

venda attur. Hann gongur vidh hessa godha lambi og er ikki varigur
venda attur. Hann gonggur vi hessa goa lambi o er itshi variyur
turn back. He goes on with this good lamb and is not aware

vidh nakadh slag,
vi naka slaea,
of any attack,

firr enn hann kemur heim í Akslarenda. Ta mötir honum ein grák-laeddur

[13] Palatalization, so characteristic of this dialect, is the chief cause of its being incomprehensible to Icelanders.

[14] It is strange to see no difference in pronunciation between *var* 'was' and *vardh* 'became'; both = '*vaer*.'

firr enn hann tshemur haim ui Akslarenda. Toa mötir honun ain groakläddur
until he comes to Akslarenda. Then meets him a grey clad

madhur sum fordhar honum vegin og ger seg inn á hann. Bádhir at berjast.
mavur sum forar honun vehgyin o dsher se inn oa hann. Boayir at beryast.
man who stops him on the road and attacks him. Both fight.

Heidhin (tí huldumadhur var tadh) baesir hinum kristna oh ber hann so
Haiyin (tui huldumavur vacar tää) bacasir hinun kristna o be-er hann so
The heathen (for the fairyman was that) beats this Christian and bears him

hadhan í Meraklettar á Beinisvöri, taett vidh eggina, har sum teir
heyan ui Meraklettar oa Beinisvöri tacatt vi edshina hacar sum tair
hence to Meraklettar on Beinsvör near the bray there where they

síga í Sjörgunshals ettir fugli.
suiya ui Shörgunshals ettir fugle.
let down ropes after birds at Shörgunshals.

Kvöldidh lídhur og eingin madhur kemur attur til húsa. Naesta
Kvöldi luiyer o aindhsin mavur tshemur attur til hiusa. Naeasta
The evening comes and no man comes back to the house. Next

mórgun fór folk úr hverjun húsi til at leita. Tey finna lambidh og stavin
mohrgun fohr fö'lk iur kveryun hiuse til at laita. Tey finna lambi o stavin
morning went the folk out of every house to search. They find the lamb and the staff

kjá Pálin heiman firi Akslarenda og geva so ivir at leita. Stutt ettir
tshoa Polin haiman firi Akslarenda o dhseva so ivir at laita. Stutt ettir
of Paul near the house at Akslarenda and so cease to search. Shortly afterwards

berst Pálin í dreymi firi konu sína, bidhur hana ikki leita saer og sigir
berst Polin ui dreymi firi konu suina, biyur hana itshi laita saear o siyir
appeared Paul in a dream to his wife, begs her not to search for him and tells

henni frá öllum sum til hevur borist vidh saer—at hin heidhni hevur
henni froa ödlun sum til hevur borist vi saear—at hin haini hevur
her about all which had happened to him—that that heathen has

tikidh og boridh seg í Meraklettar. Seint á sumri funnu neytakonur
titshi o bori seh ui Meraklettar. Saint oa sumri funnu nehtakonur
taken and carried him to Meraklettar. Late in the summer found the milk-

mannin deydhan úti á Flöum, blodhnaknan og bádhar iljaskógvarnar
mannin dehyan iuti oa Flöun, blohnaknan o boyar iljaskohgvarnar
ing women the man dead out on Flöun, stark naked and both (his) foot-soles

brendar undir honum. Bódh var sent attur til húsa og hann vardh fördhur
brendar ondir honun. Boh vaear sent attur til hiusa o hann vaear förur
burned under him. Word was sent back to the house and he was fetched

heim og grivin.
haim o grivin.
home and buried.

Grammatical Sketch.

The following paradigms will illustrate the declension of the indefinite adjective with the noun and the definite article with adjective and noun, all in comparison with the similar modern Icelandic declensions:

Indefinite Adjective.

Faeroe (phonetic).	*Icelandic.*[13]
N. *govur mavur*	*godhur madhur* 'a good man'
G. (*gohs mans*)	*godhs manns*
D. *govum manni*	*godhum manni*
A. *govan mann*	*godhan mann*

[13] The modern Icelandic phonetics are as follows: *a* as in 'father'; *e* as in 'met'; *i* as in 'pin'; *o* as in 'more'; y as i in 'pin'; *æ* = *i* in 'pine'; *ö* as *i* in 'sir'; *á* as *ow* in 'how'; *í* as *ee* in 'meet'; *ó* as *ow* in 'blow'; *ú* as *oo* in 'fool'; *u* as French *u*; *ý* as *ee* in 'meet'; *dh* = *th* in 'this'; medial *f* = *v*, except in connection with another consonant when it = *b*; *h* is always breathed, even before *j* (*hj*) and *k* (*hv* = *khv*); *j* = cons. *y*; *l* is very soft, but *ll* = *ddl*; *nn* = *ddn*; *r* final = almost *rs*, but = trilled *r* in the beginning and middle of words; *rn* = *ddn*; *th* = *th* in 'think.' Icelandic is spoken with a curious whispering tone, quite unlike Faeroese.

Pl. N.	*govir menn*	*godhir menn*
G.	(*gora manna*)	*godhra manna*
D.	*govum monnum*[16]	*godhum mönnum*
A.	*govar menn*	*godha menn*
N.	*go kona*	*godh kona* 'a good woman'
G.	(*gorar konu*)	*godhrar konu*
D.	*govari konu*	*godhri konu*
A.	*gova konu*	*godha konu*
Pl. N.	*govar konur*	*godhar konur*
G.	(*gora kona*)	*godhra kvenna*
D.	*govum konum*	*godhum konum*
A.	*govar konur*	*godhar konur*
N.	*gott badn*	*gott barn* (pr. *badn*) 'a good child'
G.	(*gohs badns*)	*godhs barns*
D.	*govum badni*	*godhu barni*
A.	*gott badn*	*gott barn*
Pl. N.	*go bödn*	*godh börn* (pr. *bödn*)
G.	(*gora badna*)	*godhra barna*
D.	*govum bödnum*	*godhum börnum*
A.	*go bödn*	*godh börn*

Definite Article Prefixed and Suffixed with Weak Adjective and Noun.

	Faeroe (phonetic).	*Icelandic.*
N.	*hin goyi mavur-in*	*hinn godhi madhur* (*-inn*) 'the good man'
G.	(*hins gova mans-ins*)	*hins godha manns* (*mannsins*)
D.	*hinum gova manni-num*	*hinum godha manni* (*manninum*)
A.	*hin gova mann-in*	*hinn godha mann* (*manninn*)
Pl. N.	*hinir govu menn-inir*	*hinir godhu menn* (*mennirnir*)
G.	(*hinna govu manna-na*)	*hinna godhu manna* (*-na*)
D.	*hinum govu monnu-num*	*hinum godhu mönnum* (*mönnu-num*)
A.	*hinar govu menn-ina*	*hina godhu menn* (*mennina*)
N.	*hin gova kona-n*	*hin godha kona* (*-n*) 'the good woman'
G.	*hinar govu konunnar*	*hinnar godhu konu* (*konunar*)
D.	*hini govu konu-ni*	*hinni godhu konu* (*konunni*)
A.	*hina govu konu-na*	*hina godhu konu* (*-na*)

[16] Note the absence of *umlaut* in *F*. Note also that the indef. article *einn* is never used in Icel., but often in F.

Pl. N. *hinar govu konur-nar*	*hinar godhu konur* (*-nar*)
G. *hina govu kona-na*	*hinna godhu kvenna* (*kvennana*)
D. *hinum govum konunum*	*hinum godhu konum* (*konunum*)
A. *hinar govu konur-nar*	*hina godhu konur* (*-na*)
N. *hitt gova badn-i*	*hidh godha barn* (*-idh*) 'the good child'
G. (*hins gova badns-ins*)	*hins godha barns-* (*ins*)
D. *hinum gova badn-inum*	*hinu godha barni* (*barninu*)
A. *hitt gova badni*	*hidh godha barn* (*-idh*)
Pl. N. *hini govu bödn-ini*	*hin godhu börn* (*börnin*)
G. *hinna govu badna-na*	*hinna godhu barna* (*barnana*)
D. *hinum govu bödn-unum*	*hinum godhu börnum* (*börnunum*)
A. *hini govu bödn-ini*	*hin godhu börn* (*börnin*)

Here it should be noted that the original genitive has practically disappeared in F. colloquial, having been replaced by the analytical form with the prep. *kjá* (*tshoa*), as *húsidh kjá pabba* (*hiusi tshoa pabba*) 'the father's house.' This tendency is the same as that seen in mod. Bulgarian, which has practically discarded all the complicated Slavonic case-endings in favor of prepositions. Another new and striking form of the gen. in personal names is very much used in the Faeroes at present, especially in Straumoy, viz., *Yoakups-sar boatur* 'Jacob's boat'; *Annu sa bouk* 'Anna's book.' As Haegstad remarks (*Vestnorske Maalföre*, p. 137), this seems to remind the observer of the West-Norse gen. with the poss. *sin*, as *Jakob sin baat; Anna si bok*, but it is really quite different in every respect, as the F. form employs the indeclinable *sa*. This form seems to have its origin in the many F. personal names which end in *-s* in the nom. preceded by a vowel, and have a gen. in *-ar*, of which combination the indeclinable *sa* is probably a corruption. In connection with the above paradigms the following facts should be observed: Note in the F. masc. indef. adj., the acc. pl. appearance of *-ar* as compared with Icel. *-a*. In the F. fem. indef. adj., note the insertion of the *a*-helping vowel in the dat. sg. *-ari;* Icel. *-ri*. In the F. neut. indef. adj., observe the retention of *-m* in the dat. sg., as *-um* (pr. *-un*); Icel. *u*.

In the F. definite declension, the suffixed article may be and usually is retained with the prefixed article *hin, hin, hit*. This phenomenon occurs also in modern Swedish: *den goda*(*e*) *mannen* 'the good

man.' In Icel., however, if the definite prefixed article is used, the suffix may not be employed; thus in F. one may say: *hin govi mavurin*, but in Icel. either *hinn godhi madhur* or *godhi madhurinn*. Note in the F. def. neuter dat. sg., the *-um* (pr. *-un*) ending: *hinun gova badninun*, not kept in Icel. and the F. nom. acc. *hini govu bödnini* as compared with Icel. *hin* and *börn-in*.

In spite of the apparent similarity shown by the above comparison between F. and Icel., the difference of the F. phonetics makes this dialect phonetically very distinct. This fact may be better illustrated by a phonetic comparison between the respective

PERSONAL PRONOUNS AND NUMERALS.

Faeroe.	*Icelandic.*	*Faeroe.*	*Icelandic.*
eg (*eh*)	*jeg* (*jekh*) 'I'	*tú*	*thú* (hard *th*) 'thou'
mín (*muin*)	*mín*	*tín* (*tuin*)	
maer (*mār*)	*mjer* (*myer*)	*taer* (*tār*)	*thín*
meg (*meh*)	*mig* (*mikh*)	*teg* (*tekh*)	*thjer* (*thyer*)
			thig (*thikh*)
vit or *vaer* (*vār*)	*vidh* 'we'	*tit* (or *taer*; pr. *tār*)	*thidh*[17] *thyer* 'you'
okkara (or *osara*)	*okkar*	*tykkara*	*ykkar ydhar*
okkum (or *os*)	*okkur*	*tykkum*	*ykkur ydhur*
okkum (or *os*)	*okkur*	*tykkum*	*ykkur ydhur*
hann	*hann* 'he'	*hon*	*hún* (*huhn*) 'she'
hans	*hans*	*hennar*	
honum	*honum*	*henni* (*henne*)	*hennar*
hann	*hann*	*hana*	*henni*
			hana
tadh (*tāā*)	*thadh* 'it'		
tess	*thess*		
tí (*tui*)	*thví*		
tadh (*tāā*)	*thadh*		

Faeroe.

Masc.	Fem.	Neut.
teir (*tair*)	*taer* (*tāār*)	*tey* (*teh*)
teirra (*tairra*)	*teirra* (*tairra*)	*teirra* (*tairra*)
teimum (*taimun*)	*teimum* (*taimun*)	*teimum* (*taimun*)
teir (*tair*)	*taer* (*tāār*)	*tey* (*teh*)

[17] The polite form of the 2 p.

Icelandic.

Masc.	Fem.	Neut.
their (*thehr*)	*thaer* (*thair*)	*thöi* 'they'
theirra (*thehra*)	*theirra*	*theirra* (*thehra*)
theim (*thehm*)	*theim* (*thehm*)	*theim* (*thehm*)
their (*thehr*)	*thaer* (*thair*)	*thöi*

ORDINALS TO TWENTY.

Faeroe.	*Icelandic.*	*Faeroe.*	*Icelandic.*
ain	*ehdn*	*to-ulv*	*tólv*
tvair	*tvehrs*	*trettan*	*threttaun* (hard *th*)
tridshir	*thrirs*	*fyuhrtan*	*fyórtaun*
fuira	*fyórirs*	*fimtan*	*fimtaun*
fimm	*fimm*	*sextan*	*sextaun*
säks	*seks*	*sehtshan*	*söitshaun* (or *setshaun*)
sheh	*shö*	*otshan*	*autshaun*
otta	*autta*	*nuitshan*	*nitshaun*
nuidshe	*níu*	*tshuvu* (cf. Swed. *tjugu*, pr. *tshügü*)	*tuttughu*
tuidshu	*tíu*		
edlivu	*edlevu*		

The F. verbal forms are similar to those of Icel. The variation will be apparent from a few examples.

VERBAL FORMS.

Present.		*Imperfect.*	
Faeroe.	*Icelandic.*	*Faeroe.*	*Icelandic.*
eg renni	*jeg nem*	*eg rann*	*jeg nam*
tú rennur	*thú nemur*	*tú rannst*	*thú namst*
hann rennur	*hann nemur*	*hann rann*	*hann nam*
vit renna (or *rennum*)	*vidh nemum*	*vit runnu* (*-m*)	*vidh námum*[18]
tit renna (or *rennidh*)	*thidh nemidh*	*tit runnu* (*-dh*)	*thjer námudh*
teir renna	*their nema*	*teir runnu*	*their námu*

The subjunctive with its characteristic *-i* occurs in both idioms, as F. pres. sbj. *renni* for all persons; Icel. *naemi*, *naemir*, *naemi*, *naemum*, *naemudh*, *naemu*.

[18] The imperf. pl. form in both dialects takes the *o-ablaut* whenever possible.

It will be observed that F. has lost the inflections especially in the pres. pl. and throughout the subjunctive.

Finally in this connection, the following comparison between F., Icel., and the "Norse-Norse" idiom as used today by the extreme partisans of "true Norwegian" in Norway may prove of interest.[19]

F. *hikk at köttinum; hann er heldur luiti duir, myukhäärdur lóafottur*
I. *littu au köttinn; hann ers fremurs lítidh dirs, myukhairdhur laugfütturs*
N. *sho po katten; han er ain heldur lite dür, myukhäärd lohgfött*
Look at the cat; he is a rather little animal; soft-haired, low-footed

F. *o-ch halolanggur. Täädnar eru stuttar o-ch kunne itshi kreppast so väl*
I. *okh medh langga róvu. Täädnars eru stuttars okh geta ekkyi bekht sikh ains vel*
N. *o med ai lang rova. Tärne er stutte o kann ikkye kreppa seh i hop so gott*
and with a long tail. The toes are short and cannot bend themselves as well

F. *sum finggrar okkara.*
I. *okh finggurnirs au oss.*
N. *som fingrarne vore.*
as our fingers.

Music.

The Faerings have preserved a wealth of dances accompanied by many ancient dance songs which are characteristic of these islands. They also still use a number of narrative songs of the saga variety, some of which even refer to episodes connected with King Pepin, the father of Charlemagne (*Pippingur oa Fraklandi*, 'Pepin of Frankland') and of other early monarchs. Most of these airs have a melody variation of only four or five tones and are believed to go back to a prehistoric origin. A specimen of a still popular dance song given below may be of interest, in closing this brief sketch of a people who have kept their nationality in much the same manner as has been done in Iceland, owing to a thousand years of comparative isolation.

[19] Haegstad, *Vestnorske Maalföre*, pp. 188–190; F. phonetics by Miss Mikkelsen; Icel. phonetics by Mr. Kristian Armansson, an Icelandic student in Copenhagen.

Text of Song (Phonetic).

Bruhnsveins Vuisa.[20]

Hoyr tä Myödlkvuit svara mär bluit me yungga
Hearken Snowwhite answer me blithely with the young folk

E ruiya ui lund at biya mär vuiv
I ride to the wood to beg me a wife

Adl favurt lyovar muin tungga
So fair rings my lay (tongue)

Lystir me ui dans goa me yungga
I long to go in the dance with the young folk

Bruhnt är muit silke hoar; myödlkvuit so äri e shoal
Brown is my silken hair. I myself am snow-white

Adl favurt lyovar muin tungga
So fair rings my lay (tongue).

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Jakob Jakobsen, *Diplomatarium Faeroense* with historical treatise, Thorshavn and Copenhagen, 1901.

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Jakob Jakobsen, *Fuglekvöje* (Birdsongs), Copenhagen (S. L. Möller), 1892.

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M. Raymond Pilet, *Rapport sur une Mission en Islande et aux Isles Féroë*, Paris (Ernest Leroux), 1915.

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[20] As sung by Miss Mikkelsen from Hj. Thuren's *Folkesangen paa Faeröerne* p. 85.

Hjalmar Thuren, *Folkesangen paa Faeröerne* (Höst Sönner), Copenhagen, 1908.

Hjalmer Thuren, *Dans og Kvaddigtning paa Faeröerne* (Dance and Song poetry in the Faeroes) (Höst Sönner), Copenhagen, 1901.

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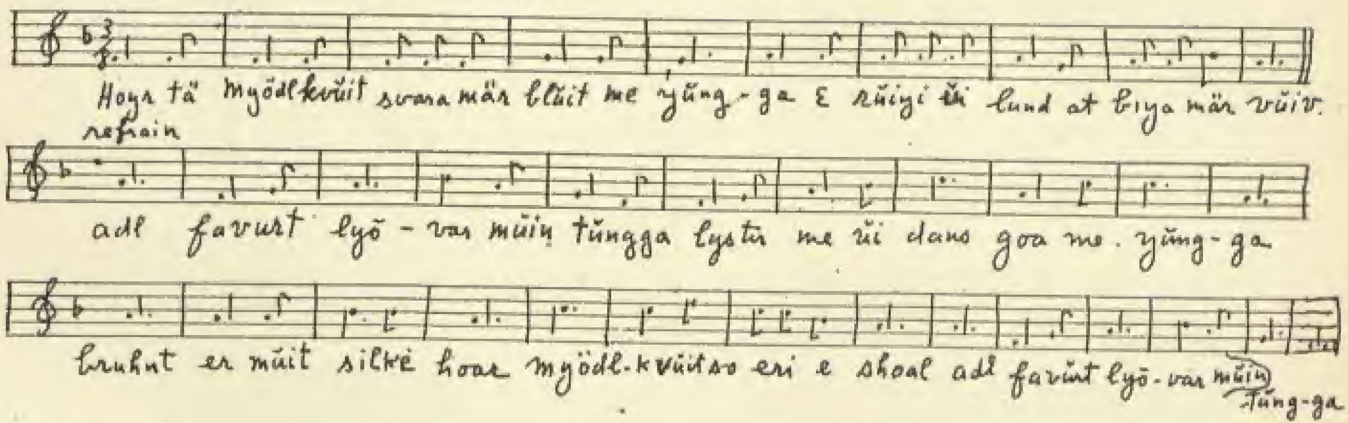
LEGATION OF THE UNITED STATES OF AMERICA,
COPENHAGEN, March 11, 1922.

Bruhnsvains vüisa



MINUTES.

MINUTES.

Stated Meeting, January 6, 1922.

William B. Scott, Sc.D., LL.D., President, in the Chair.

The decease was announced of

Henry Turner Eddy, Ph.D., Sc.D., LL.D., on December 11, 1921, æt. 77.

Mr. Alfred Flinn, of New York, read a paper entitled "Engineering Research and Vicarious Tests."

Stated Meeting, February 3, 1922.

William B. Scott, Sc.D., LL.D., President, in the Chair.

The decease was announced of

Rt. Hon. James Bryce, Viscount Bryce, O.M., LL.D., D.C.L., at Sidmouth, England, on January 22, 1922, æt. 84.

Mr. H. Goodwin, Jr., read a paper entitled "The Meaning of Superpower to the Public."

Stated Meeting, March 3, 1922.

William B. Scott, Sc.D., LL.D., President, in the Chair.

The decease was announced of the following:

James W. Holland, M.D., at Philadelphia, on February 10, 1922, æt. 73.

John C. Branner, Ph.D., Sc.D., LL.D., at Stanford University, on March 1, 1922, æt. 71.

Dr. Walton Brooks McDaniel read a paper on "Ancient Survivals in Modern Greece and Italy."

Stated General Meeting, April 20, 21, and 22, 1922.

Opening Session, Thursday Afternoon, April 20.

Hampton L. Carson, M.A., LL.D., Vice-President, in the Chair.

Prof. Paul Shorey and Prof. E. Washburn Hopkins, recently elected members, subscribed the Laws and were admitted into the Society.

The following papers were read:

"Our Contradictory Economic Policy," by E. M. Patterson, Ph.D., Professor of Economics, University of Pennsylvania. (Introduced by Prof. Emory R. Johnson.)

"The Distribution of Human Ability in Europe," by Ellsworth Huntington, Ph.D., Research Associate in Geography, Yale University. (Introduced by Mr. Henry G. Bryant.)

"George Hammond and Robert Liston—British Ministers in Philadelphia," by J. F. Jameson, Carnegie Institution of Washington, Department of Historical Research, discussed by Mr. Carson and Dr. Hays.

"The Threefold Trinity," by E. Washburn Hopkins, Ph.D., LL.D., Professor of Sanskrit and Comparative Philology, Yale University.

"The Use of Devices for Indicating the Vowel Length in Latin," by John C. Rolfe, Ph.D., Professor of Latin Language and Literature, University of Pennsylvania.

"A Sketch of the Modern Faeroe Dialect," by J. Dyneley Prince, Ph.D., U. S. Envoy Extraordinary and Minister Plenipotentiary to Denmark.

Friday, April 22.

Executive Session, 10 O'Clock.

William B. Scott, Sc.D., LL.D., President, in the Chair.

Dr. Lawrence J. Henderson, a newly elected member, subscribed the Laws and was admitted into the Society.

The President delivered his Annual Address as follows:

"Under the new laws of the Society adopted last autumn, it becomes the duty of the Chair to present to the Society at this executive meeting a brief report concerning the activities of the Society for the preceding year and the state of its property and personnel.

"The total number of members of the Society on December 31 last was 476; 402 being residents of the United States and 74 foreign

members. Of the American members, 121 reside within 30 miles of the hall. which is about 25 per cent. of the total membership; 281 reside beyond 30 miles of the hall, almost 60 per cent. of the membership. The foreign membership is about 15½ per cent. For some years past, by direction of the Society, no foreign members have been elected, because it was thought that the percentage of such members was already too high; and it was resolved that until the number of foreign members was reduced to 50 no further additions to the list should be made, save in case of unusual emergency. During the year 18 members died and 15 were elected.

"During the year eight stated meetings were held, and there was one special meeting for the purpose of receiving Madame Curie, who had been elected to membership in 1910. She exhibited and described one of the instruments which she and her husband had used in accurate measurement of radio-activity. At the same meeting the Board of City Trusts of Philadelphia presented to Madame Curie one of the John Scott medals and awards.

"At the annual General Meeting, in April last, 108 members were present and 54 papers were read. The Henry M. Phillips prize of $2,000 was awarded to Mr. Qunicy Wright, of Minneapolis, for his essay entitled 'The Control of the Foreign Relations of the United States.'

"The report of the Curators is to the effect that the collections of the Society, both in the building and those which are deposited elsewhere, are in good condition.

"The Treasurer's report has been printed and distributed for your information. It may be briefly summarized as follows:

"The assets in the Treasury, at par, of securities is $310,000, one half of which belongs to the general fund and the other half to special funds. The securities are in the custody of the Pennsylvania Company for Insurance on Lives and Granting Annuities, as our agent for the collection of income.

"The general income of the Society, for other than trust fund purposes, is about $13,000.

"The Building Fund, which is in the hands of the Girard Trust Company, as Trustee, amounted at the last report to $72,216. The

income of this fund is added to the principal as it accrues. Since this report the Society has received a legacy from the estate of our late member, Ferdinand J. Dreer, of $3,537.46, which the Society has voted should be added to the Building Fund, bringing this up to about $75,750.

"The Publication Committee has been much hampered by the strike of the printers, which has affected nearly the whole country. Two numbers of the *Proceedings* were published in 1921, and two other numbers, completing the volume, were published early in 1922. Under the new laws, the committee has appointed Dr. Alexander C. Abbott to be the Editor of the Society's publications.

"The question of scientific publication is one of the most difficult problems which scientific men have to face at the present time. All avenues of such publication are choked with contributions which deserve publicity, but which can not have it under present conditions. Of especially distressing urgency is the question of adequate illustrations for the papers published. The extravagantly inflated prices which have been brought about by the war make such illustrations almost unattainable, and lithographic plates are only for multimillionaires. In my judgment, the Society could do nothing so serviceable to science in this country as raising a fund which should enable it to increase its publications extensively, and especially to provide suitable and proper illustrations. This service to our scientific public would be well rewarded in the increase of the Society's prestige and reputation.

"The Library Committee reports the total number of volumes in the Library at the end of 1921 to have been 69,687, of which the accessions of the year were 960 volumes. In addition to these, there are 4,069 maps. There are sent to the Library 1,833 serial publications, of which 1,255 are received in exchange for the Society's publications. The subscriptions and exchanges for all countries, with the exception of Russia and the Balkan States, have been resumed; the gaps created by the war have been largely filled up and measures are being taken to fill the gaps that still remain.

"This gives you in brief form a statement of the Society's activities and plans; and I would, in closing, particularly urge upon you the necessity of doing something to increase publication and provide

illustrations. The Society has largely increased the sum to be devoted to publication in the ensuing year, but this will do nothing more than put us back where we were before the inflation of prices began, and does nothing to increase our facilities. This, I take it, is the most important problem that we have to solve."

Morning Session, 10.45 O'Clock.

The following papers were read:

"Novæ as Variable Stars," by E. E. Barnard, D.Sc., LL.D., Professor of Practical Astronomy, University of Chicago, Astronomer of Yerkes Observatory.

"The Message of a Meteorite," by Monroe B. Snyder, of the Philadelphia Observatory.

"The Effect of Diurnal Variation of Clock Rates Upon Longitude Work," by R. H. Tucker, C.E., Lick Observatory, Mt. Hamilton, California, discussed by Prof. Ernest W. Brown.

"Discussion of a Kinetic Theory of Gravitation, II.; and Some New Experiments in Gravitation. Second Paper," by Charles F. Brush, Sc.D., LL.D., Cleveland.

"Absorption Spectra and Ionization Potential in Dissociated Gases," by Karl T. Compton, Ph.D., Princeton University. (Introduced by Dr. Augustus Trowbridge.) Discussed by Profs. Goodspeed and Snyder.

"Recent Developments in Vacuum Tubes and Their Use," by J. H. Morecroft, Columbia University, New York. (Introduced by Dr. A. W. Goodspeed.)

"A Primary Standard of Light," by Herbert E. Ives, Ph.D., New York City.

"Surface Equilibrium of Certain Colloid Solutions," by P. Lecomte du Noüy, M.D., of the Rockefeller Institute. (Introduced by Dr. Carrel.) Discussed by Prof. Scott.

Afternoon Session, 2 O'Clock.

HAMPTON L. CARSON, M.A., LL.D., Vice-President, in the Chair.

Messrs. Douglas Campbell, John J. Carty, and Moses Gomberg, recently elected members, subscribed the Laws and were admitted into the Society.

The following papers were read:

"Notes on the Ecology of the Clovers (*Trifolium*)," by John W. Harshberger, A.B., Ph.D., Professor of Botany, University of Pennsylvania.

"The Cytoplasm in Development and Heredity," by E. G. Conklin, Ph.D., Sc.D., Professor of Biology, Princeton University.

"The Supposed Serial Arrangement of the Genes, and its Relation to Theories of Crossing-over in Inheritance," by H. S. Jennings, Ph.D., LL.D., Professor of Zoölogy and Director of the Zoölogical Laboratory, Johns Hopkins University.

"The Relation of the Retinal Image to Animal Reactions," by G. H. Parker, S.D., Prof. of Zoöl., Harvard Univ.

"Parallel Mutations in Œnothera," by George H. Shull, B.S., Ph.D., Professor of Biology, Princeton University.

"Some Climatic and Topographic Characters in the Rings of the Yellow Pines and Sequoias of the Southwest," by A. E. Douglass, D.Sc., Director of Steward Observatory, University of Arizona. (Introduced by Dr. D. T. MacDougal.)

"The Probable Action of Lipoidals in Growth," by D. T. MacDougal, M.A., Ph.D., LL.D., Director of the Department of Botanical Research, Carnegie Institution of Washington, Tucson, Arizona, discussed by Dr. Ernest W. Brown.

"A Possible Explanation of Eocene Climates," by Edward W. Berry, Professor of Palæontology, Johns Hopkins University.

Friday Evening.

Mr. Vernon Kellogg spoke on "The Power and Impotence of Man."

Saturday Morning, April 22.

Executive Session, 10 O'Clock.

William B. Scott, Sc.D., LL.D., President, in the Chair.

Prof. Edward Capps and Dr. T. B. Osborne, recently elected members, subscribed the Laws and were admitted into the Society.

The Society proceeded to the election of Officers and Councillors

for the ensuing year and for members, and the Tellers subsequently reported the following as having been elected:

President.

William B. Scott.

Vice-Presidents.

Arthur A. Noyes,
Henry Fairfield Osborn.
Hampton L. Carson,

Secretaries.

Arthur W. Goodspeed,
Harry F. Keller,
John A. Miller.

Curators.

William P. Wilson,
Henry H. Donaldson.

Treasurer.

Eli Kirk Price.

Councillors.

(To serve for three years.)

Lafayette B. Mendel,
Herbert S. Jennings,
William W. Campbell,
Robert A. Millikan.

(To fill an unexpired term in the class of 1920.)

Felix E. Schelling.

Members.

Residents of the United States.

Charles Elmer Allen, Ph.D., Madison, Wis.
Rollins Adams Emerson, Sc.D., LL.D., Ithaca.

Worthington C. Ford, A.M., Litt.D., Cambridge, Mass.
Frederick E. Ives, Philadelphia.
Irving Langmuir, Ph.D., Schenectady, N. Y.
Roland S. Morris, A.B., LL.D., Philadelphia.
George William Norris, A.B., M.D., Philadelphia.
Charles Lee Reese, Ph.D., Wilmington, Del.
Harlow Shapley, Cambridge, Mass.
Henry Skinner, M.D., Philadelphia.
James Perrin Smith, A.M., Ph.D., LL.D., Palo Alto, Calif.
Charles Cutler Torrey, A.M., Ph.D., D.D., New Haven.
Robert De Courcy Ward, A.M., Cambridge, Mass.
Henry Stephens Washington, A.M., Ph.D., Washington.
David Locke Webster, Stanford University, Cal.

The resignation of Dr. W. M. L. Coplin was received and accepted.

Morning Session, 10.30 O'Clock.

William B. Scott, Sc.D., LL.D., President, in the Chair.

The following papers were read:

"Hyracodonts from the Big Badlands of South Dakota," "The Small Entelodonts of the White River Oligocene," by W. J. Sinclair, Ph.D., Department of Geology, Princeton, N. J. (Introduced by Prof. W. B. Scott.) Discussed by Prof. Scott.

"Lithology of the White River Sediments," by H. R. Wanless, Princeton University. (Introduced by Prof. C. H. Smyth.)

"Lava Domes and Their Composition in the Malay Archipelago," by H. A. Brouwer, M.I., Professor of Historic Geology and Paleontology, Technical High School, Delft, Netherlands; Exchange Professor at Univ. of Michigan. (Introduced by Prof. W. B. Scott.)

"Application of Biophysical Researches to Medical Problems," by George W. Crile, M.D., and Hugo Fricke, Ph.D., Cleveland.

"Experiments in Epidemiology," by Simon Flexner, M.D., D.Sc., LL.D., Director of Laboratories, Rockefeller Inst. for Medical Research.

"Fishes Used in Guayaquil for Mosquito Control Against Yellow Fever," by Carl H. Eigenmann, Ph.D., Professor of Zoölogy, Indiana University.

"The Carbonic Acid of the Blood in Health and Disease," by Lawrence J. Henderson, M.D., Asst. Professor of Biological Chemistry, Harvard Medical School.

"Some Recent Experiments Concerning the Nature of the Function of the Kidney," by A. N. Richards, M.D., University of Pennsylvania. (Introduced by Dr. H. H. Donaldson.)

"The Biblical Manna," by Paul Haupt, Ph.D., LL.D., Professor of Semitic Languages, Johns Hopkins University.

Afternoon Session, 2 O'Clock.

William B. Scott, Sc.D., LL.D., President, in the Chair.

Prof. Wilder D. Bancroft, a recently elected member, subscribed the Laws and was admitted into the Society.

The President announced the award of the Magellanic Premium to Paul R. Heyl and Lyman J. Briggs, of the U. S. Bureau of Standards, for the invention of "The Earth Inductor Compass," and presented to them the Magellanic Gold Medal.

Messrs. Heyl and Briggs exhibited a model of the compass and described it, and Lieutenant J. Parker Van Zandt, official representative of the U. S. Army Aviation Service, made some appreciative remarks on the invention.

The President made the following remarks on the retirement of Dr. Hays from the Secretariat after a continuous service of twenty-five years:

"The members of the Society will have noted, with feelings akin to dismay, that for the first time in many years the name of Dr. I. Minis Hays is absent from the list of Secretaries, Dr. Hays having retired in spite of the many urgent requests that he should continue in office.

"It is difficult to understand what the Society can do without Dr. Hays, for he has been its mainspring and driving force for years past. The Society was in a moribund, or at least a comatose, condition when

he began his work; and Dr. Leidy about that time described it to me as an example of 'how a Society that pretended to demand some measure of distinction from those whom it elected to membership was doomed to sterility and inactivity by that very fact.' Now this has all been changed; and the annual General Meeting of this Society, which was inaugurated on Dr. Hays's suggestion, is universally regarded as of great importance. The late Professor Pickering, when Vice-President of the Society, once remarked from this chair that this annual meeting had become, in his opinion, the most important and the most interesting scientific event of the year.

"All this we owe to Dr. Hays, and it is fitting that the Society should recognize his invaluable services. I will therefore call upon the Secretary to read the Resolution which he has prepared for this occasion."

Secretary Goodspeed offered the following minute:

"Whereas we have all known for some time of the intended retirement at this time of our senior Secretary and Librarian, Dr. I. Minis Hays, and

"Whereas we are all deeply impressed with the inestimable value of his long and faithful service to our honorable Society, we, the members, hereby unite to record our deep appreciation of all Doctor Hays has done to forward the interests of the Society, and our sincere regret that he is to be no longer with us in his former official capacity, together with our earnest desire that he may yet for many years remain our willing adviser in the conduct of the affairs of the Society."

This minute was duly seconded and unanimously adopted.

The following papers were read:

"Description of Earth Inductor Compass," by Paul R. Heyl and Lyman J. Briggs. (Introduced by the President.)

"Symposium on the Age of the Earth,"

"From the Geological Viewpoint," by T. C. Chamberlin, Sc.D., LL.D., Professor of Geology and Director of the Walker Museum, University of Chicago.

"From the Palæontological Viewpoint," by John M. Clarke, Ph.D., Sc.D., LL.D., Director of the Department of Science and State Museum, Albany, N. Y.

"From the Astronomical Viewpoint," by Ernest W. Brown, M.A., Sc.D., Professor of Mathematics, Yale University.

"From the Radioactive Viewpoint," by William Duane, Professor of Biophysics, Harvard Medical School.

DISCUSSION OF A KINETIC THEORY OF GRAVITATION, II; AND SOME NEW EXPERIMENTS IN GRAVITATION.

SECOND PAPER.

By CHARLES F. BRUSH.

A year ago I had the honor to present to this Society the first paper under the present title. The second, or experimental part of that paper, appeared to show that equal masses of zinc and bismuth, also of iron, and presumably of other metals and substances, do not have exactly equal weights; in other words, that the *mass-weight* relationship differs in different substances. For instance, a given mass of bismuth appears to weigh a little more than an equal mass of zinc; or, to state it differently, as in the pendulum experiments following, gravity acts more strongly per unit of mass on bismuth than on zinc.

When established, this is a fundamental fact of supreme importance; and the present paper is devoted wholly toward its confirmation, leaving theoretical consideration for future papers.

As the following experiments relate only to the *mass-weight* difference between zinc and bismuth, I shall, for simplicity, refer to it as the "zinc-bismuth effect."

Of the several experiments detailed in the first paper, that of the two gravity pendulums is the simplest and most easily understood, the most accurate and reliable, and has brought out most suggestions of possible sources of error. The latter are all considered in the following review and extension of the pendulum experiments; and the conditions as to length of rods, and weight and shape of bobs, have been widely varied. In *every* case the former zinc-bismuth effect has been confirmed, and in *no* instance has there been any negative or equivocal result.

PROC. AMER. PHIL. SOC., VOL. LXI, M, NOV. 16, 1922.

Review and Extension of Gravity Pendulum Experiments Described in First Paper.

Most of this work has been purposely delayed until after publication of the first paper,[1] in the hope that if any source of large error had been overlooked at the time of writing that paper, it would be pointed out by some one. Nothing of this sort has been brought to my attention.

The next four or five pages, as far as Table I., are in substance quoted from the first paper.

Plate I. shows the pendulum apparatus as originally installed, together with driving clocks at the top, added later for long-continued observations.

A starting cradle, moving in guides on the low table just below the cylindrical zinc and bismuth bobs, serves to start the pendulums swinging exactly together in any desired amplitude. After pushing the bobs sufficiently to the left, the cradle is suddenly withdrawn to the right, leaving the bobs free. This device is entirely satisfactory in performance.

A horizontal thick plate of hardened steel is very firmly bolted to the lower flange of a heavy iron I beam imbedded in the masonry of the ceiling and walls of the room. The plate is dropped 6.5 cm. below the beam by cylindrical iron spacers through which the bolts pass, and is carefully leveled. Near one edge of the upper face of the plate is a long shallow $\vee$ groove of 90° angle, with a slightly rounded bottom carefully ground straight and polished after the plate was hardened.

From this plate hang two exactly similar pendulums of about 2.284 m. effective length and 15.2 cm. apart. Each pendulum rod, except for a few centimeters at each end, is of mild steel, perfectly straight, and 1.6 mm. diam. Both rods were cut from the same specimen, so as to have the same temperature coefficient. The upper 20 cm. of each rod is 0.4 cm. diam. round steel with fine screw thread and thumb nut on its upper part. The thumb nut has eight radial holes for a long brass pin, the whole adapted to effect very fine adjustment of pendulum length. The thumb nut rests on the horizontal face of a 60° triangular "knife-edge" of hardened steel

[1] Proc. Am. Phil. Soc., Vol. LX., No. 2, 1921.

through which the rod passes. The upper part of the rod is slightly flattened on one side by grinding, and a thumb screw in one end of the knife-edge block bears against the flattened side of the rod and clamps it firmly in the block after each length adjustment is made. The knife-edge, ground true and sharp, rests in the plate groove above described, while the rod passes downward through an opening in the side of the plate.

Each pendulum rod terminates at its lower end in a straight brass rod 13 cm. long and 0.4 cm. diam. A perfectly straight horizontal steel pin passes loosely through the brass rod near its lower end, and on this pin the cylindrical bob, or weight as I shall hereafter call it, rests.

Fig. 1 shows the upper and lower parts of one pendulum in detail, with the bismuth weight in place.

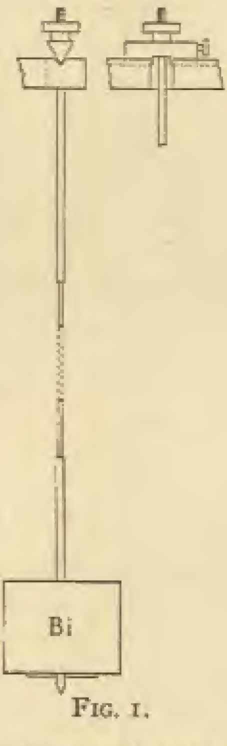


FIG. 1.

The brass rod at the lower end passes just freely through the weight, and accurately in its axis. A weight is easily removed from

either pendulum by lowering it after the pin is withdrawn, and another weight may be substituted by reversing the procedure. While this is being done the pendulum rod is kept taut by another temporary, radially slotted, lead weight applied just above, and resting on the upper end of the brass rod. Thus the weights forming the bobs of the two pendulums may readily be exchanged without disturbing anything else.

The weights to be compared, bismuth and zinc in the first instance, were made very accurately the same in height, and with upper and lower ends as nearly plane and parallel as possible, by careful grinding on a perfectly flat surface.

It is essential that the centers of gravity of the weights be exactly the same distance above their supporting pins. To assure this, each weight was adjusted to have its center of gravity exactly midway between its upper and lower ends by the following procedure: The pendulums having been started swinging with a definite amplitude and brought to synchronism by length adjustment, one of the weights was turned over; this at first resulted in loss of synchronism at the same amplitude. Then, as indicated, the upper or lower cylindrical portion was slightly reduced in diameter by turning off or sandpapering in the lathe. Again the pendulums were synchronized, and again the same weight was turned over and synchronism tested. This process was repeated again and again with each weight until either could be turned over without affecting synchronism in the slightest observable degree. In making these adjustments very minute departure from synchronism could be detected in half an hour at the turning points of the swing.

Instead of making the cylinders the same in diameter, they were made approximately the same in *weight*, about 1.377 kg., so that when they were exchanged the length of the pendulum rods would not be affected. Otherwise it would have been necessary to apply corrections for the elastic modulus of the rods and for their effect on the *center of oscillation of the weights* with every exchange. The latter correction would have been very large, and liable to error.

Finally, the zinc and bismuth pendulums were adjusted to synchronism as perfectly as possible in 40-minute runs with initial amplitude of 35 cm. As it turned out, the bismuth pendulum was then

materially *longer* than the zinc one. It was the whole aim of the pendulum experiments to detect and measure this difference if it existed.

Next, the weights were exchanged, so that in effect the bismuth pendulum was now the *shorter* one by *double* the former difference. On again starting the pendulums, at the former amplitude, loss of synchronism was easily observable in 2 minutes—the bismuth gaining. In half an hour the bismuth gain was very large. In the same and other forms this experiment was repeated many times, and *always* with the same unequivocal result.

Equality of air resistance was effected by attaching small paper vanes to opposite sides of the bismuth normal to the line of swing, of such size as to produce air damping equal to that of the zinc as shown by equal time loss of amplitude. The necessary vanes are very small.

It appears from this experiment that the earth's gravitation field, which is here the accelerating force, grips the bismuth more strongly per unit of mass than it grips the zinc per unit of mass; in other words, a given mass of bismuth appears to *weigh more* than the same mass of zinc. Apparently the length of a standard seconds pendulum may depend on the material of which it is made.

The greater diameter of the zinc cylinder slightly lowers its center of oscillation; and this accounts for about 10 per cent. of the effect above described, as determined by elaborate experimentation which need not be detailed here, and which was approximately verified by computation.

A pair of high-grade, weight-driven clock movements were next added to the apparatus, as shown in the upper part of Plate VII., and adapted to drive the pendulums continuously at an amplitude of 13 cm.

After synchronizing the zinc and bismuth pendulums at this amplitude, the zinc and bismuth weights were exchanged as heretofore described. Then they were started exactly together and allowed to run until they were again exactly together, the bismuth having thus gained two full beats. Half the elapsed time was taken as the value of *one* beat gain.

Again the pendulums were synchronized, the zinc weight now

being on the pendulum formerly occupied by the bismuth weight; then the weights were exchanged as before, the pendulums started together, and allowed to run until the bismuth had gained two beats as formerly. This procedure was for the purpose of verifying the first finding and to expose any considerable difference there might be in the performance of the driving clocks. No such difference was found; yet for verification the same procedure was followed in the next experiments.

A cylinder of very pure iron was next prepared, of exactly the same height, and approximately the same weight as the zinc and bismuth cylinders, and adjusted for center of gravity with the same care.

The iron weight or cylinder was then compared with the zinc weight and with the bismuth weight, with the same care used in comparing the zinc and bismuth as above described. The iron gave results intermediate between those of zinc and bismuth.

Table I. shows the performance of the zinc-iron, the iron-bismuth, and the zinc-bismuth combinations. The measurements of time required to gain one beat check and confirm each other remarkably well.

As the pendulums make about 2,388 oscillations per hour, the bismuth gains one beat, or oscillation, in about 17.432; but as before pointed out, the real zinc-bismuth effect is only half of this, say, one

TABLE I.

Zinc-Iron	15½ hrs. 18½ "	Mean 17 hrs.
Iron-Bismuth	13 hrs. 12½ " 13½ "	Mean 13 hrs.
Zinc-Bismuth	7 hrs. 20 min. 7 " 16 "	Mean 7.3 hrs

Zinc — Iron — Bismuth

17 hrs. — 13 hrs.

7.3 hrs.

Reciprocals:

$$\frac{1}{17} + \frac{1}{13} = \frac{1}{7.36}$$

part in 35,000. Thus a *weight-mass difference* effect appears fairly well established and is impressive.

Effect of Unequal Weight of Bobs.—In the foregoing experiments the zinc cylinder or bob weighed about 7 grams *less* than the bismuth (about ½ per cent.); and it was known that this would tend to "speed up" the zinc and make the period difference or "zinc-bismuth effect" appear *less* than if the bobs were equal in weight. But it was thought that the effect of this weight difference was so small that it would not be worth while to delay the experiments several days to correct it, though computation would have shown differently.

Last summer a review and extension of the pendulum experiments was commenced, and the first thing done was correction of the weight deficiency of the zinc cylinder. This was effected by placing a tightly fitting equatorial band of thin sheet zinc on the cylinder and driving it slightly one way or the other until perfect adjustment of center of gravity was had, as and for the reason heretofore explained. Finally, the zinc band was secured from accidental displacement by several very small drops of solder at its edges. As it turned out, the banded zinc, instead of being 7 grams lighter than the bismuth as before correction, was then about one tenth gram heavier than the bismuth. This latter difference is trivial and was not corrected.

The effect of equalizing the weight of the zinc and bismuth was unexpectedly large. Instead of the bismuth gaining one beat in 7 hours 18 minutes, as shown in Table I., it now gained a beat in 5 hours 11 minutes.

The reason for the large disturbing effect of any considerable weight difference between the bobs is perfectly clear. It is due to the distribution of weight in the equal pendulum rods. Obviously, if we could have stretchless and weightless rods, even large weight differences between the bobs, whatever they were made of, would have no disturbing effects except those due to different diameters, which would change their center of oscillation and air resistance.

But the center of oscillation of a pendulum rod alone is *very far* above that of its bob, and it is the resultant center of oscillation of the combined rod and bob that we have to deal with. Clearly, then, if we reduce the weight of a bob, we raise the center of oscillation of the combination and thus shorten its period.

The new and enlarged value of the zinc-bismuth effect found, as above described, clearly does not invalidate the usefulness of Table I. as a check on the accuracy of the work done before correcting the weight of the zinc bob. If the new value were introduced in a similar table, it would simply increase the zinc-iron effect (less hours for one beat gain of iron) as well as the zinc-bismuth effect, and show that the iron is more closely related to bismuth than to zinc in effect producing qualities.

Inequality of bob weights is much the largest and most dangerous source of error I have found in the pendulum experiments; dangerous because it is not very obvious (no one has pointed it out), and if overlooked may lead to wholly erroneous results—even reversing the sign of the zinc-bismuth effect. I have said so much about it for the guidance of others who may wish to try pendulum experiments for themselves.

The next step was to make a new pair of zinc and bismuth bobs as nearly as possible exactly like the original pair, except that the zinc cylinder is slightly larger in diameter so as to have the required weight without banding, and the bismuth cylinder purposely has a coarser crystalline structure.

The new castings were made like the old ones, in molds consisting of vertical iron cylinders somewhat larger in diameter than the finished bobs, with an asbestos paper tube extending above. Thus the castings were chilled at their lower end, and rather rapidly cooled throughout.

But before machining, the bismuth casting only, was packed in molding sand in an iron box and gradually heated in a large electric muffle furnace above the melting point of bismuth, and then allowed to cool *very* slowly through the freezing stage of the metal by suitably reducing the heating current in the furnace. This procedure surely resulted in a crystalline structure of very large grains.

Finally, the new cylinders were worked to weight, *length*, parallelism of ends, and adjustment of center of gravity with all the care and patience exercised with the earlier cylinders. The weights of the new and old cylinders are now as follows:

Weight of old zinc	1,383.899	grams
Weight of new zinc	1,383.893	"
Weight of old bismuth	1,383.795	"
Weight of new bismuth	1,383.798	"

Large weighings like these are made with a 2-kilogram Becker balance of the most improved type, easily sensitive to a tenth milligram with full load.

The members of each pair of cylinders are made so closely alike in weight because they are to be superposed on the pendulum rods; and any difference in weight would shift the center of gravity of the pair.

The purpose of the second pair of bobs is, first, to permit comparison of bobs of the same metal—I am often asked to do this; and, second, by superposing the zinc or the bismuth, to provide bobs of double the former weight and very different in shape. It was not expected, however, that even such large variation of weight and shape of bobs would throw any new light on the subject; and it did not, as will appear. This will be gratifying to such of my friends as have been bothered with doubts in these respects.

Next, the old and new bismuth bobs were mounted on the two pendulum rods; synchronized, exchanged, and period difference measured; again synchronized, exchanged, and period difference measured, all as formerly done with *zinc* and bismuth bobs. In addition to this, each bob, after exchanging, was turned over and tested again so as to average out any small errors that might have been left when center of gravity adjustments were made. In these experiments the small paper vanes on the old bismuth cylinder were eliminated in effect by turning them into the line of swing.

In *every* instance the new bismuth *gained* over the old bismuth, but not much. Averaging all the measurements, the mean gain was about .6 mm. per hour measured in middle of swing with a reading telescope. Whole amplitude was about 11.4 cm. Hence somewhere about 190 hours would have been required for a full beat gain, such as takes place in about 5 hours when zinc and bismuth are compared. Therefore, we may call the new bismuth-bismuth effect about 2½ per cent. as large as the normal zinc-bismuth effect.

The above shows too much difference in behavior between old and new bismuth cylinders to be attributed to experimental errors. It seems not unlikely, then, that there is a real *mass-weight* difference between the old and new bismuth cylinders. It is believed that this difference is due to difference in crystalline structure. If this is the

true explanation, it is exceedingly interesting in showing that physical condition affects the relation of weight and mass in bismuth, and presumably in other substances. Certain other experiments on very different lines, which will be detailed in a future paper, appear to support this view.

But the old and new *zinc* cylinders, when compared with similar care, also showed a similar, though smaller, difference in behavior, the new zinc being faster than the old. This may be due to the fact that the new bob was made of electrolytic zinc, presumably almost chemically pure, while the old one was made of ordinary commercial zinc, presumably not nearly so pure.

The latter and subsequent experiments were greatly facilitated by using a reading telescope located in the plane of the pendulum wires when at rest, and focussed sharply on the nearer wire just above the brass rod which carries the bob. This leaves the farther wire hazy in outline, so that the two wires are easily distinguished as they cross the field of view. When the pendulums are in synchronism, the wires are superposed as they cross the center of the field. In this way very small departures from synchronism can be detected in a few minutes, which would require many times longer to grow sufficiently to be seen with the unaided eye.

Next, both zinc cylinders were mounted on one pendulum rod, the new one above; and both bismuth cylinders on the other rod, the new one above, all as shown in Plate VIII. (the plate, however, showing much shorter pendulum rods). The old bismuth cylinder had its small paper vanes turned normal to line of swing, while the new bismuth cylinder carried a very thin aluminum vane normal to swing, of computed area sufficient to make its air resistance equal that of the new zinc cylinder.

Here we have bobs of double the former weight, and very different in shape. Of course, the periods are slightly shortened.

Then these double bobs were fully compared in the same manner as were the former single bobs. The closely same results are shown in Table II.

Both pendulum rods were next shortened nearly a meter, and a suitably higher table was placed below them, as shown in Plate VIII.

FIG. 1. Titanotherium beds topography as exposed in Indian Creek, Big Badlands. Note the rounded character of the hills. The Sheep Mountain section of Oreodon and Leptauchenia beds is seen in the distance to the left.

FIG. 2. Section of the Oreodon beds in Corral Draw, Big Badlands. The upper nodular level is marked (*), and the lower zone of rusty nodules or the red layer (**). Photo from the collection of the late S. W. Williston.

FIG. 1. Section of the Leptauchenia volcanic ash beds in a canyon of Sheep Mountain, Big Badlands. The top of the columnar vertical weathering in the picture is the top of division (*a*) of the Leptauchenia beds, as described in the text. Above this is the "white ash layer" of Matthew, division (*b*) of the text. This appears lighter colored than division (*a*), and does not show vertical columnar weathering.

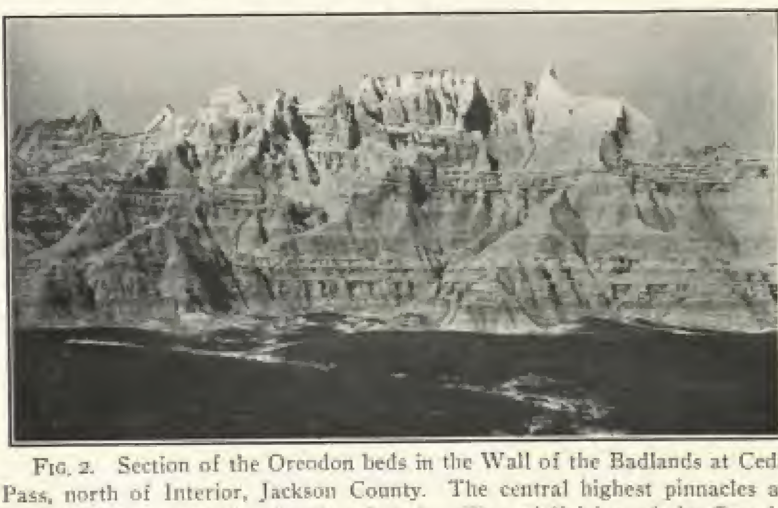


FIG. 2. Section of the Oreodon beds in the Wall of the Badlands at Cedar Pass, north of Interior, Jackson County. The central highest pinnacles are capped by Leptauchenia volcanic ash beds. The subdivision of the Oreodon beds here into upper clays, upper nodular layer, middle clays, and lower nodular layer are clearly shown, especially in the right half of the picture. The upper nodular layer is marked (*) and the lower nodular layer (**).

TABLE II.

LONG PENDULUMS.

	Time in Minutes Required for Bi to Gain One Beat. Synchronized. Bi North.	Bi South.	Mean.	Single Swings for Bi to Gain One Beat.	Double Zn-Bi Effect Observed $\times 10^{-7}$.	Zn-Bi Effect Observed $\times 10^{-7}$.	Radius of Gyration Effect Computed $\times 10^{-7}$.	Zn-Bi Effect Corrected $\times 10^{-7}$
Single Bobs								
Zn 7 gms. deficient in wt.	436	440	438	17,450	57.3	28.6	9.0	19.6 = 1/51,000
Zn and Bi wts. equal	314	308	311	12,390	80.7	40.3	9.0	31.3 = 1/32,000
New Zn, New Bi.	301.5	314.5	308	12,270	81.5	40.7	9.0	31.7 = 1/[illegible]
Double Bobs	298.7	306.8	303	12,110	82.6	41.3	9.2	32.1 = 1/[illegible]

SHORT PENDULUMS.

Single Bobs, New Zn and Bi.	221.2	214	217.6	11,220	89.1	44.6	25.2	19.4 = 1/[illegible]
Double Bobs	179	193.2	186.1	9,630	103.8	51.9	26.0	25.9 = 1/[illegible]

PERIODS.

Single Beats per minute.

Long Pendulums,	Single Bobs	...	39.834
" "	Double Bobs	...	39.993
Short Pendulums,	Single Bobs	...	51.546
" "	Double Bobs	...	51.752

The single and double bobs on the shorter rods were compared as before, with results shown in lower part of Table II. But reversal of the larger and smaller time quantities in columns 1 and 2 lead to strong suspicion of experimental errors, probably due to insufficient care in exchanging the bobs after synchronizing. Unfortunately, as the last experiments were made quite recently, there has not been time to repeat them.

Effect of Greater Diameter of Zinc Bobs on Center of Gyration.—In last year's paper this effect, with the long pendulums, was estimated to account for about 10 per cent. of the whole zinc-bismuth effect as experimentally found, and somewhat more than this by computation. But the fact that this correction should be doubled

when applied to the single (or real) zinc-bismuth effect was inadvertently overlooked.

Table II. shows that this correction (subtractive) for either single or double bobs on the long pendulums is 22 per cent., and, of course, grows rapidly as the pendulums are shortened.

It is seen that the very consistent results of single and double bobs of equal weight on the long pendulums indicate a zinc-bismuth effect of about one part in 32,000. But in all the pendulum and other experiments the end sought is not so much quantitative accuracy as qualitative certainty. In the present stage of the general investigation it matters little whether the true zinc-bismuth effect is one part in ten thousand or a hundred thousand; the great point is to make *sure* that it is *something* tangible. This is the purpose of my long course of experimentation.

Another, though small, correction might be made in Table II. (additive), due to the fact that the pendulum rods are of different material from the bobs.

Effect of Unequal Air Resistance.—I am often asked if performance of the pendulum experiments *in vacuum* might not greatly affect, or even obliterate, the apparent zinc-bismuth effect. To this I can confidently answer *no*. As I see it, the only thing to be gained by working in vacuum would be elimination of unequal air resistance due to the unequal diameters of the zinc and bismuth cylinders.

But this is easily compensated when working in air by attaching to the smaller bismuth cylinder small and very light vanes of paper or very thin aluminum normal to the line of swing of suitable area experimentally found or computed. This was always done in the foregoing experiments.

Careful experiments have shown that wholly uncompensated inequality of air resistance would increase the apparent zinc-bismuth effect only about 10 per cent. The effect of the air vanes on the bismuth is to slightly lengthen its period.

It is well known that wind pressure on a cylinder, normal to its axis, is approximately *half* that on a plane surface of a width equal to the diameter of the cylinder. Therefore, if we were to place on either double bob cylinder shown in Fig. 2 a plane air vane normal

to line of swing, and as high and broad as a single cylinder, it would double the air resistance of that pair.

The diameters of the zinc and bismuth cylinders are, respectively, 72 and 61 mm., and their common height (single) 48 mm. Incidentally it will be seen that a zinc cylinder has 18 per cent. greater diameter than a bismuth cylinder, and presumably 18 per cent. more air resistance.

An air vane of thin cardboard 48 mm. high was placed on each double pendulum bob as shown in Fig. 2; that on the zinc 50 mm. wide, and on the bismuth 83 mm. wide, as shown by the dotted lines. The vanes were adapted to be turned into the line of swing, which

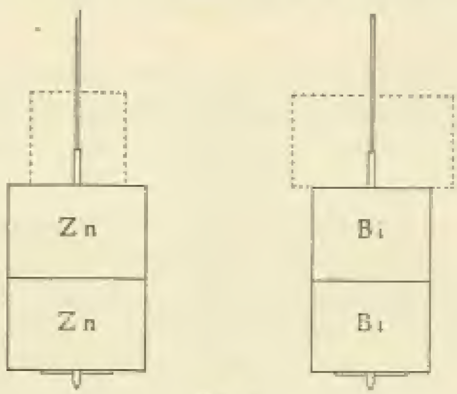


Fig. 2.

was equivalent to removing them, or normal to the swing, which would give them full effect.

It is easily seen that either vane, when turned normal to line of swing, *doubles* the air resistance of its pendulum as compared with the other pendulum having its vane in the line of swing.

Prior to adding these very large vanes, the pendulums had been synchronized and their bobs exchanged as in the last entry of Table II., showing the mean time required for the bismuth to gain one beat was 186 minutes.

When the air resistance of the zinc was thus made *double* that of the bismuth, the time required for the latter to gain one beat was 137 minutes. When these conditions were reversed, giving the bis-

muth double the air resistance of the zinc, the time required for the bismuth to gain one beat was 274 minutes.

Thus it is seen that when we give the zinc more than *five times* its uncompensated normal excess of air resistance the apparent zinc-bismuth effect is increased only one half; and when we give the bismuth 100 per cent. *excess* of air resistance, instead of its normal 18 per cent. deficiency, the apparent zinc-bismuth effect is still very considerable after correcting for radius of gyration effect.

I hope these *reductio ad absurdum* experiments make it clear that no error need accrue from swinging the compensated pendulums in air instead of in vacuum. It is true that in vacuum the periods of both pendulums would be slightly shortened; but they would be equally shortened.

Effect of Local Distortion of the Earth's Magnetic Field.—It has been suggested that distortion of the earth's field by large masses of iron in the laboratory and in the building structure may have something to do with the observed zinc-bismuth effect.

There are no considerable masses of iron nearer than about 2 meters from the swinging bobs, and their distorting effect must be very nearly uniform in the small space in which the bobs swing; so that both bobs will be affected substantially equally. However, to test out the effect of *great* distortion of the earth's magnetic field in the immediate vicinity of the bobs, and *very unequally* for the two, the following experiment was made:

After synchronizing single zinc and bismuth bobs on the short pendulums, a large cylinder of very pure soft iron 31 cm. high and 7 cm. diameter, shown on right front corner of table in Plate VIII. (never left there, however, during other experiments), was placed vertically with its axis in the plane of the two pendulums at rest, and with its axis 10 cm. away from that of the nearer bob; and later in the same relation with the other bob. Then the bobs were started swinging together.

In each case the nearer bob was accelerated, but only about 11 per cent. as much as bismuth is always accelerated when the bobs are exchanged after synchronizing. The amplitudes were slightly diminished. When a glass cylinder of similar size was substituted for the iron, no disturbance of period was observed.

It is clear, then, that in the customary absence of iron anywhere near the bobs they can not be unequally affected by the earth's magnetic field to any appreciable extent.

Necessity of Very Rigid Support for the Pendulums.—When two pendulums are hung from the same support, there is a well-known tendency of each to influence the other by slight motions imparted to the common support. It was to counteract this tendency that the common support of the pendulums used in these experiments was made so very rigid.

The following experiments were made to test the efficiency of this provision, and the double bobs were used because of their greater weight, and the short pendulums because of their greater angular amplitude.

The pendulums were so closely synchronized that no appreciable change occurred in 3 hours when swinging together. Then they were started as nearly as possible in opposite phase, and again allowed to swing 3 hours. Again they were started a half phase apart, and allowed to swing 3 hours. In neither case was there any observable change in phase relation during the three hours' run. Three hours' run was deemed sufficient, because that was about the time required for the bismuth to gain a whole beat when the bobs were exchanged.

Thus it is seen that the pendulums do not observably affect each other because of their common support.

To find to what extent, if any, the pendulums affect each other by the interaction of the air eddies which they set up, the bobs were exchanged as usual, started together, and time required for *two* beats gain of bismuth taken. Then they were again started, and immediately a large glass plate was placed vertically midway between the pendulums in the line of swing, and resting on the table. Again the time required for two beats gain of the bismuth was measured. This procedure was repeated without, and with the interposed glass plate, and with the following results:

TIME OF TWO BEATS BISMUTH GAIN.

Without glass plate	357 minutes
With glass plate	352 "
Without glass plate	360 "
With glass plate	353 "

The mean effect of the glass is to speed up the bismuth 6 minutes, or about 1.7 per cent.

This small discrepancy in periods may be largely due, and almost certainly is partly due, to the effect of the glass plate on the slight air currents unavoidably present in the room. The plate shields one of the bobs from these currents, and by reflection accentuates their effect on the other bob, except when, if ever, the currents happen to be closely in the plane of the plate.

Hence we may conclude that the effect of interacting air eddies set up by the bobs is negligibly small.

The foregoing review and extension of the pendulum experiments, unless some at present unsuspected source of large error is discovered, leads me to believe firmly that there is a real and very considerable mass-weight difference between zinc and bismuth. This belief is strongly supported by the torsion pendulum experiments of last year, and by several other lines of experiment now in progress, one of which I shall briefly describe:

A most carefully designed and constructed instrument of precision has been built for comparing the velocities of freely falling bodies in two aluminum containers, alike in size, shape, and smoothness of surface. These are loaded to exactly the same weight, to equalize air resistance effects, and dropped simultaneously, side by side, from exactly the same height always. Have had no trouble with this part of the apparatus.

After falling about 120 cm. the containers strike a pair of targets exactly equidistant from their starting points. This adjustment is made by loading the containers with the same metal and reversing their position again and again while adjusting the targets until the containers strike them simultaneously. The mechanism is such that the containers push the targets aside after striking them.

When the containers are loaded with zinc and bismuth, the one containing bismuth strikes its target just a little before the zinc one arrives. The time difference must be of the order of a hundred thousandth part of a second, and the beautiful timing mechanism detects this easily.

But in using this apparatus a persistent collateral phenomenon developed, which promises to be of great interest. This is being in-

vestigated, pending which a description of it, and of the complete apparatus and its timing device, as well as the general results obtained, is reserved for a future paper. The pertinent fact here is that bismuth apears to fall a little faster than zinc, thus confirming the pendulum experiments.

CLEVELAND,
April, 1922.

LITHOLOGY OF THE WHITE RIVER SEDIMENTS.

Investigation aided by a grant from the Marsh fund of the National Academy of Sciences.

By HAROLD R. WANLESS.

(Plates IX. and X.)

(*Read April 22, 1922.*)

I. Introduction.

Since 1847, when the first White River fossil was described by Dr. Joseph Leidy, the abundant and varied fauna of this series, as exposed in the Big Badlands of South Dakota, has made it a center of interest for paleontologists and collectors. During the field seasons of 1920 and 1921 the writer has worked in this area with Professor W. J. Sinclair with the object of tracing the detailed faunal changes and determining, from the lithology of the beds, the conditions of sedimentation through the successive formations which make up the White River series. The work has been carried on in the Big Badlands south and southwest of the town of Scenic in Pennington County, and along the Wall of the Badlands north of Interior in Jackson County.

The earliest investigators assumed that most of the Tertiary mammal-bearing beds of the west were lake deposits, and that the excellent preservation of the skulls and skeletons they afforded was due to protection by water from subaërial weathering and disintegra-

tion. Hatcher was among the first to suggest the possibility of a flood-plain of fluviatile origin instead of lacustrine, and evidence in recent years has been accumulating which supports his hypothesis. We are now able to recognize river-channel, fresh-water pond, sheet-flood, and eolian elements in the series. In the present investigation the writer has endeavored to draw from the lithologic characteristics of the sediments the sources of the material, the methods of transportation and deposition, and the physiographic and climatic conditions under which the White River faunas lived and evolved.

II. Generalized Section of the Big Badlands.

To make the stratigraphic relations of the various types of sediments more clear, a generalized columnar section of the Big Badlands is presented in Figure 1, *C*, correlated with a section of the Oreodon beds from the Wall of the Badlands near Interior, about 40 miles to the east, and with Wortman's 1893[1] section, which has been generally used as the standard section for the district and is now somewhat in need of revision. The datum plane of correlation used is the top of the lower zone of rusty nodules or "Red Layer," as defined below. The stratigraphic elements of the White River series may be briefly indicated as follows:

1. *Titanotherium Beds.*—This zone consists of clay beds with numerous large sandstone channels. Thin limestone lenses are frequent throughout, but caliche zones are rare or absent. The subdivision of the Titanotherium beds into Upper, Middle, and Lower zones in Wortman's section is based on the supposed zonal distribution of titanotheres presenting different stages of horn development, as worked out by Hatcher.[2] A corresponding stratigraphic or lithologic subdivision has not yet been recognized and it is doubtful whether such will ever be possible. The thickness of the Titanotherium beds in the sections measured by our expeditions varies from 110 to 132 feet, which is somewhat less than Wortman's figure of 180 feet, though there are doubtless thicker sections than those measured by the writer, for the Titanotherium beds vary greatly in thickness from point to point, resting as they do on the very irregular erosion surface

[1] *Am. Mus. Bull.*, 5, 1893, pp. 98–9.
[2] *Am. Naturalist*, March, 1893.

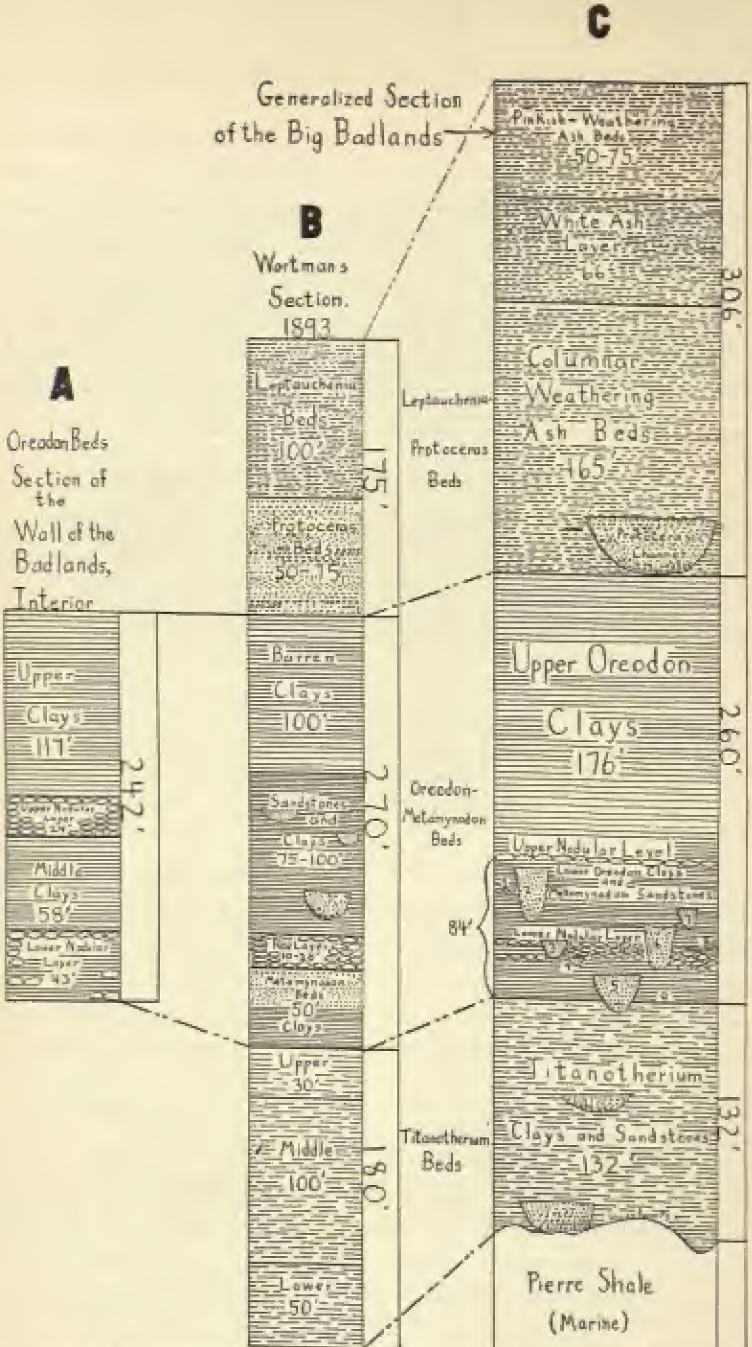


FIG. 1. Correlation diagram of sections of the White River formation. Section C: (1) algal ball horizon, Quinn Draw; (2) Metamynodon channel sandstone, Quinn Draw; (3), (4), (5) and (6) Metamynodon channel sandstones in Corral Draw; (7) Metamynodon channel sandstone, Little Corral Draw; (8) Solid sheet of algal limestone, Little Corral Draw; (9) Thin sheet of silicified limestone at base of Oreodon beds.

of the Pierre shale. The lower part of the Titanotherium beds consists of blue and lavender clays made up entirely of reworked Pierre shale and affording calcareous nodules with cone-in-cone structure containing remanie Pierre shale fossils.[3] This lower zone is discussed below in connection with ground water circulation. The channels and limestone lenses are so numerous and, apparently, local that their stratigraphic positions have not been indicated in the generalized section.

2. *Oreodon-Metamynodon Beds.*—(*a*) Wall of the Badlands section. The Oreodon or Middle White River series as generally developed consists essentially of clays with local limestone lenses, interrupted by channels of coarse sandstone containing *Metamynodon*. In the "Wall of the Badlands" section near Interior (Fig. 1, *A*) they are divided into: (*a*) lower zone of rusty nodules and clays; (*b*) intermediate clay zone; (*c*) upper zone of nodules, more generally greenish than rusty; (*d*) upper clay zone. (See Plate II, Fig. 2.) The upper zone of nodules as here developed is absent in the Big Badlands and the same term is there applied to a thin caliche zone which is remarkably persistent over at least 200 square miles and may extend farther yet, but does not occur, or has not been identified, in the Interior section.

(*b*) Big Badlands section (Fig. 1, *C*). The base of the Oreodon beds in the Big Badlands is defined by discontinuous thin bands or lenses of silicified limestone or chert, ranging where present from two inches to one foot thick. This division plane was mentioned by Darton[4] when he first defined the Chadron and Brule formations. Where the limestones are absent the line of change in the weathering between the Oreodon and Titanotherium beds is used for the contact plane. The profiles developed on the Oreodon clays, as a rule, have steep slopes and a more angular appearance, while the Titanotherium beds weather with gentler slopes and more rounded surfaces. Plates I. and II. bring out this contrast clearly.

In the same area, the lower zone of rusty nodules, or so-called Turtle-Oreodon layer or "red layer," is defined by Sinclair[5] as beginning

[3] *Science*, Vol. 19, p. 254, 1904.

[4] Professional Paper, 32, U. S. G. S., p. 71.

[5] PROC. OF THE AMER. PHILOS. SOC., Vol. 40, 1921, pp. 457–466.

with the algal limestone or chert band at the base of the Oreodon beds and continuing upward from 29 to 43 feet (varying with the locality), generally overlain by a more greenish clay or nodule-bearing horizon much poorer in fossils than is the red layer. The red layer consists of pinkish clay with rusty-brown caliche nodules in fairly regular bands or irregularly distributed. In the Indian Creek sections usually from one to three nodule-bearing horizons are found in it, while in parts of Corral Draw as many as ten levels of nodules may occur. Elsewhere, as on Cain Creek, six miles east of Scenic, nodules are locally absent over large areas, and the whole lower Oreodon series consists of clays. As it is often impossible to tell at what part of the lower zone the nodules are developed it has been found that the upper layer discussed at greater length in the paragraph following makes a better datum plane than the lower zone. Both of these zones are very fossiliferous and the intervening clays from the upper nodular level to the base of the Oreodon beds have also produced many important fossils. Professor Sinclair's paper on the "Turtle-Oreodon layer or 'red layer,' a Contribution to the Stratigraphy of the White River Oligocene," which has just appeared, discusses primarily the lower nodular zone, and reference may be made to it for further details.

The upper level of rusty nodules is six inches to one foot thick and 80 feet, more or less, above the base of the Oreodon beds, or 40 to 60 feet above the top of the Lower nodular zone, the variation being due to changes in the thickness of the latter. As mentioned above, this thin zone of nodules is so widely distributed over the area of the Big Badlands that it makes the best datum plane in the whole series of the Oreodon beds. It has been clearly recognized at points separated by 25 miles (Arnold's Ranch district, 12 miles southeast of Scenic and Battle Creek Canyon, 30 miles southwest of Scenic) and may be found to have a still wider distribution.

The channels which cut the lower zone of rusty nodules are characterized by the presence of the large aquatic rhinoceros, *Metamynodon*, and are therefore called Metamynodon sandstones. They cut the Oreodon clays at various levels, but are most numerous between the upper level of rusty nodules and the base of the Oreodon beds. The stratigraphic position of six of these channels is shown in Fig. 1,

Section *C*, which also gives the position of a limestone lens in Little Corral Draw and an algal ball level in Quinn Draw.

The clays above the upper nodular level are generally alternate bands of pale pink and pale green beds with very few channels and nodular layers and no limestone lenses observed. The average thickness of this clay series in the Big Badlands is 160 to 170 feet, with a variation of not more than 10 feet in three sections measured 20 miles apart. This clay zone is less interesting because of its poorer fossil content and has not as yet been studied in much detail. Around the western and southern ends of Sheep Mountain this clay series is capped by a heavy sandy nodular layer about 50 feet thick, but this layer was not observed elsewhere.

3. *Leptauchenia-Protoceras Series.*—*A.* Protoceras Beds. The name Protoceras beds has often been applied to all of the Upper White River, but should be restricted to the stream channels cutting the so-called Leptauchenia clays, the former alone carrying a Protoceras fauna. No channels over 30 feet in thickness have been observed by the writer, and Wortman's figure of 50–75 feet is probably somewhat excessive. The position of these channels is generally at or near the base of the Leptauchenia beds. As the Protoceras sandstones are only formed in stream channels, many sections contain no Protoceras beds at all, the Leptauchenia horizon resting directly on the Upper Oreodon clays. It has been supposed that the Protoceras sandstones were deposited right in the stream channels where the current was strong, while the Leptauchenia "clays" were deposited at the same time in the quieter backwaters. In many samples of Protoceras sandstones, however, water-worn concretions are found, such as are very characteristic of the Leptauchenia beds. These concretions were sufficiently consolidated at the time of the Protoceras channels to be preserved as pebbles in the sandstones formed. This shows that the Protoceras channels were actually cut through the Leptauchenia beds and are thus younger than part of the latter zone. older Leptauchenia beds and are thus younger than part of the latter zone. This suggests contemporaneous erosion of the country rock by the channel forming streams.

B. Leptauchenia Beds. The most complete section of these is in

Sheep Mountain in the Big Badlands. They are subdivided in ascending order into:

(*a*) Vertical, columnar weathering, ash beds often affording abundant nodules somewhat resembling coprolites in shape and size. Each nodule has a vertical central tube around which the nodule seems to have grown by concentric accretion. The cementing material of these nodules has not yet been investigated. The general color of this division is pale pink. Thickness, 165 feet.

(*b*) A massive, white, volcanic ash layer at the top of the zone of columnar weathering, 66 feet thick. This and division (*a*) are shown in Plate II., Fig. 1. This zone is *more* resistant to weathering than (*a*), but does not show vertical faces.

(*c*) A capping of 50 to 75 feet of pinkish nodule-bearing volcanic ash beds, exposed on the highest parts of Sheep Mountain.

This gives a total thickness of 281–316 feet for the Sheep Mountain Leptauchenia beds section. If Matthew[6] is correct in correlating the Sheep Mountain "White Ash" layer with that exposed in the Porcupine Creek section of the Lower Rosebud Miocene, about 20 miles south of Sheep Mountain, divisions (*b*) and (*c*) of the above sequence may belong rather to the Lower Rosebud than the White River. This correlation is at present tentative and must be worked out by a more thorough study of both the stratigraphy and the faunal changes.

Additional stratigraphic details, it is hoped, may be presented in a subsequent paper.

III. Method of Investigation.

The method pursued in the examination of the sediments is essentially that described by Crook.[7] A sample of 50 grams was selected for study and, in the case of the finer sediments, was crushed so that all of it passed through a one-millimeter mesh. This was done in order to allow the acid to act freely on all parts of the sediment, so that all of the calcareous cement would be dissolved. This was

[6] Matthew, *Bull. Am. Mus. Nat. Hist.*, Vol. 23, pp. 169–219, 1907.

[7] Hatch & Rastall, "Textbook of Petrology: The Sedimentary Rocks," 1913, pp. 339–414.

then treated with dilute hydrochloric acid until no effervescence followed the addition of more acid. The residue was then washed and dried at 110° C. and weighed, the difference representing the quantity of soluble cement. The residue was then placed in a beaker and water added to a level 9 cm. above the base of the beaker. This was allowed to stand 10 minutes, in which time all particles with a density of quartz or greater and a diameter greater than 0.01 mm. settled. The liquid with the mud was decanted and this process was repeated until the liquid was clear at the end of 10 minutes. The residue was then dried and weighed, the difference in weight representing the amount of mud in the sample. The residue was then decanted to remove silt. The sediment was covered with 14 cm. of water and allowed to settle for 20 seconds and decanted. This decantation was continued until the liquid was clear, some of the silt being saved for examination. The particles thus separated as silt have diameters of 0.01 to 0.1 mm. All coarser residues, when dried and weighed, represented the sand content of the sample.

In samples containing a noticeable percentage of heavy minerals, the sand residue was treated in a separatory funnel with Thoulet's solution (mercuric chloride and iodide dissolved in water) of a density of 2.95–3.0. By this method the rarer heavier minerals were concentrated and their amount and variety determined. From the heavy concentrate the magnetite was removed by an ordinary horseshoe magnet, and it was found that a fair concentrate of micas for study purposes, without attempt at quantitative separation, could be made by sliding the lighter sands repeatedly from a sheet of paper, where the mica flakes generally remained on account of their flatness.

The sands, silts, and heavy concentrates were then studied microscopically, with a series of liquids of known indices of refraction, and the individual minerals thus determined.

IV. Types of Sediments.

In the sequence of sediments composing the White River series there are: (1) Stream-channel sandstones, represented by those of the Protoceras, Metamynodon, and Titanotherium levels whose relations to the surrounding clays can, generally, be easily deciphered. Due to the superior resistance to weathering of the sandstones, the

courses of old stream channels can often be traced clearly, sometimes by the crest of a high divide, sometimes by a marked ridge of sandstones across a modern creek valley.

(2) Fresh-water limestone deposits. These occur at various horizons through the Titanotherium and Oreodon beds and represent small pond or damp meadow deposits.

(3) Sheet-flood deposits. These constitute the bulk of the sediments of the Badlands and are normally clays not thoroughly consolidated, except where they have been old land surfaces for considerable periods, and have given rise to nodular or "caliche" beds by the drawing up of ground water to the surface where it evaporates, leaving its mineral content as a cement for the clays.

(4) Volcanic ash beds. The term "Leptauchenia Clays" is inappropriate, as the Leptauchenia beds consist essentially of pumice and volcanic glass fragments, connected with the eruptive action of the Cordilleran district during its Tertiary orogenesis. That this ash is probably wind-carried and water-laid, or, at least, reworked by water into essentially horizontal beds of sheet-flood deposition, is the conclusion reached from the strongly marked horizontal bedding of these beds, and the absence of cross-bedding.

(5) Eolian deposits. There are few traces of wind-blown sands in the White River in the area investigated, but one bed has been found in the lower part of the Leptauchenia series in the Sheep Mountain section which seems to be mainly a dune sand, and the sands of the Rattlesnake Butte sand calcite locality are apparently of dune origin, as described by the writer in another paper.[8]

These various types of deposition may now be discussed in somewhat greater detail.

The Channel Sandstones.

Numerous typical sandstones from Titanotherium, Metamynodon, and Protoceras channels were examined. The amount of calcareous cement varied from 14 to 39 per cent., averaging about 25 per cent. In one sample of coarse Titanotherium sandstone, in addition to 26 per cent. of calcareous cement, the grains were found to be embedded in a matrix of chalcedony which may have constituted 10 or 15 per

[8] American Mineralogist, Vol. 7, No. 5. May, 1922.

cent. of the whole. Another sandstone sample, not directly connected with a channel, was nearly uncemented and crumbled readily in the hand. This showed only 2 per cent. of calcareous cement.

As for mineral composition, the grains show very definitely their derivation from the pre-Cambrian schists and pegmatites of the Black Hills. The quartz grains are ordinarily between 80 and 90 per cent. of the total sand and show frequent mineral inclusions, mainly biotite and tourmaline, which, according to Sherzer,[9] are, as a general rule, characteristic of gneisses and schists. The coarser sandstones contain a larger proportion of fresh, non-kaolinized feldspar than is found in the finer silts and clays of the series. Of the grains determined, microcline is the most abundant species; pink orthoclase is next in abundance and the plagioclase feldspars are quite rare, albite and oligoclase-andesine being determined. The feldspars averaged from 5 to 15 per cent. of the sands. Of the micas, white mica and brown mica (muscovite and probably biotite) are the most abundant, but green and black micas also are frequently present. The micas are most abundant in sands with small grains and evidently were laid down in quieter shallows away from the main current of the streams. They may total 1 or 2 per cent., but rarely more.

The heavy residues are more abundant and of larger grain in the coarser conglomeratic phases of the sandstones which contain occasional large quartz and feldspar pebbles. These were evidently formed where the current was strongest, as the finer sediment could not come to rest. The most abundant of the heavy minerals is a pink garnet which generally occurs in good rhombic dodecahedrons or combinations of these with tetragonal trisoctahedrons, or in sharp angular fragments of these crystals. In one sample where the heavy concentrate constituted 3 per cent. of the total sand, the garnet was nearly 90 per cent. of the concentrate. The other abundant heavy mineral is tourmaline, either the black massive variety of pegmatites (schorl) or very small, long prisms of dark brown and green color. The tourmaline never occurs in rounded grains in these sediments. Magnetite is present in some sands to 0.1 per cent. or occasionally more. It is usually in very fine well-rounded grains resembling in

[9] *Bull. Geol. Soc. America*, Vol. 21, 1910, p. 638.

appearance small particles of shot. Staurolite as angular, orange to brown grains is almost always present with the garnet. Other minerals identified in smaller quantities are: Augite (light green grains), rare; pyrite, very rare; hematite, apatite, and amblygonite (lithium phosphate). Other metallic nonmagnetic grains are perhaps cassiterite, wolframite, or columbite, all of which are common minerals of the pegmatite dikes of the Black Hills. In the coarser sandstones are found small pebbles of quartz-schist, quartz-sericite schist, and garnet-biotite schist, and a few pebbles of very fine-grained slates.

The evidence of the composition of the sandstones, as to the derivation of their material, is in full accord with the evidence of the direction of the channels, which lead away from the central granitic core of the Black Hills. Garnet, staurolite, the various schist pebbles, and quartz with inclusions point to derivation from the Algonkian schists and slates of this central core, and the tourmaline, amblygonite, and cassiterite (if present) point to derivation from the pegmatite dikes of the same district. The microcline, augite, and apatite are accessory constituents of the sands and may well have had a similar source.

The channels are evidently of different ages, as shown by their different stratigraphic positions in the accompanying sections, and some horizons seem to be cut by no channels through their whole visible extent. The upper nodular layer is an example of this.

A sample of the channel sand from the stream bed of Battle Creek, near Keystone, in the center of the Black Hills pre-Cambrian core, and within 5 miles of Harney Peak was collected and examined. It corresponds very closely in mineral composition to the stream-channel sands of the White River, but differs in the proportion of minerals present, and is generally of coarser grain, containing more garnets which may reach 30 per cent. of the total sand and more schist pebbles. This would be expected, as the stream is flowing through a garnet-mica schist terrane where the sample was collected. This may be regarded as confirmatory evidence of the Black Hills derivation of the material of the channel sandstones.

Fresh-Water Limestones.

At various levels through the Titanotherium and Lower Oreodon beds are thin sheets or lenses of white limestones. These are some-

times in the form of algal ball levels made up of a series of flattened, oval-shaped balls which are often quite persistent. Elsewhere thin sheets of silicified limestones occur in which the original organic character has been almost destroyed by replacement by secondary silica. Such a sheet as this is sometimes quite persistent at the contact of the Oreodon and Titanotherium beds and, as elsewhere indicated, was used by Darton in defining the contact plane of these two horizons. A third type of limestone forms a lens-shaped solid sheet a few acres in extent. This type develops a limestone as much as 3 feet thick and is quite rich in organic remains, especially the shells of cyprids. This latter type undoubtedly represents a small pond on the surface of the flood-plain. Small pelecypods and gastropods are visible in this, but the fauna has not yet been described. It is believed that one reason for the silicification of the limestone is the solution of the siliceous shells of diatoms and replacement of the lime by this dissolved silica and also the solution of the limestone by ground water and replacement by colloidal silica. The fact that the limestone was in large measure formed by the action of fresh-water algæ was first suggested by Sinclair in 1920 for the algal ball type of limestone.[10] The presence of algæ has since been recognized in the other types of limestone mentioned above. The amount of lime in these beds is very variable, from 5 per cent. in the silicified cherty layer at the base of the Oreodon beds to 95 per cent. in one of the massive lenses of limestone.

The detrital material in the limestones showed the presence of fragments of quartz, biotite and tourmaline, and of pumice needles and angular volcanic glass fragments, suggesting that the detrital deposition in the ponds was probably by wind. It may be that the thinner sheets of algal limestone do not represent pond deposits, but were formed in moist meadow lands under prairie conditions.

Nodular Layers.

The presence of nodular layers is of great interest to the palæontologist, as it is in them that vertebrate remains are best preserved. In some localities the nodules occur more or less isolated, but more often they are found as broad sheets and as such have a remarkable continuity.

[10] Amer. Philos. Soc., Vol. 40, 1921, p. 460.

The essential difference between the composition of the caliche levels and that of the other parts of the clay series is in the proportion of calcareous cement. Samples of the upper and lower zones of rusty nodules analyzed gave, respectively, 51 and 37 per cent. soluble calcareous cement. A nodular layer (not caliche) of more sandy character, but of uncertain origin, in the upper 50 feet of the Oreodon beds, exposed only in the Sheep Mountain sections, contains 30 per cent. of soluble cement. The average amount of soluble cement of the clays of the White River series is from about 5 per cent. in the Titanothere beds to 15 per cent. in parts of the Oreodon series. The caliche nodular layers were doubtless cemented at or near the surface by ground water rising by capillary attraction and depositing the calcium carbonate when it evaporated. Thus the bones of animals which happened to be near the surface at this time were protected from disintegration. The clay forming the nodular layers was originally a fine-grained flood-plain deposit which persisted as a land surface for a considerable period of time and probably indicates a period of greater aridity, by reason of the breaks in deposition, evidence of evaporation, etc. The nodular layer has in the presence of coprolites of carnivorous animals and rodent-gnawed bones absolute proof of subaërial deposition. Many of the bones found are also of a decidedly weathered appearance much as are bones of modern animals weathering on the surface of the Badlands today and occasionally buried in silt. Further proof of subaërial deposition was recently found during the preparation of a *Cænopus* skull collected in the lower zone of rusty nodules, a large number of casts of insect larval burrows being found within and around the skull, suggesting that it had lain on the surface for some time before burial and the meat had been devoured by the scavenging insects, which subsequently burrowed for pupation in the mud investing the skull. It has not been possible yet to determine to what form these insects belonged. The great continuity of the nodular layers, which has been mentioned above, implies a very level surface controlling evaporation, which was cut here and there by shallow winding stream channels, which may have carried running water only in flood time. A uniform climatic factor, probably a period of greater aridity, was one of the most im-

portant elements in the formation of the caliche nodular levels. The part which climate played in the color-banding of the Oreodon clays has not yet been determined.

The clays making up the nodular layers resemble the normal clays very closely. They contain very finely divided silt from the decomposition of the feldspars of the Black Hills, and small angular fragments of quartz of a size smaller than the lower limit of rounding by water. Flakes of brown mica are frequently seen. Fragments of pumice and volcanic glass were recognized in all samples studied and sometimes make up an appreciable per cent. of the total clastic material. These were wind-carried and water-laid, as demonstrated by the marked horizontal bedding of the nodular layers and the absence of cross-bedding.

Volcanic Ash Beds.

The Leptauchenia beds consist largely of a well-consolidated white fine-grained rock, which weathers with a marked vertical columnar appearance, giving rise to sheer cliffs up to 200 feet high. This seems to consist very largely of volcanic glass and pumice. The pumice shows numerous capillary tubes in parallel arrangement which are sometimes deformed by flowage (indicated by strain polarization). There are also throughout the mass long hair-like needles with capillary tubes in the center resembling superficially Pele's hair. With the material of volcanic derivation there are also small fragments of mineral matter of the texture of silts. The grains are not rounded and are mostly too small to be rounded by water. Quartz and biotite are the most common of these mineral fragments, and feldspars and tourmalines were also recognized. A sample of the sand from the White Ash layer, division (*b*) of the beds as described above, which contains about 90 per cent. glass or pumice fragments, was analyzed by Professor A. H. Phillips, of Princeton University, with the following result:

SiO_2	68.21	MgO	1.13
TiO_2	0.32	K_2O	4.41
Al_2O_3	10.97	Na_2O	3.13
Fe_2O_3	2.89	P_2O_5	none
FeO	0.13	$H_2O > 110°$	5.01
MnO	0.12	$H_2O < 110°$	3.05
CaO	1.08		100.45

Calculated in terms of norms as described by Iddings,[11] this glass is a rhyolite with the following standard mineral composition:

Orthoclase	28.36	Diopside	2.38
Albite	28.82	$MgSiO_3$	2.00
Anorthite	2.78	Ilmenite	0.60
Quartz	31.93	Hematite	3.13

In some specimens of these ash beds small cavities and irregular cracks are lined with needle-like crystals of a zeolite, apparently mordenite, of the composition $(Ca, Na, K)_2Al_2Si_{10}O_{24} \cdot 6\frac{1}{2}H_2O$, with a mean index of refraction of 1.465 and a very low double refraction, about .005. This was probably formed as a result of slight hydrometamorphism by alkaline solutions deriving their soluble content from the volcanic ash on which they reacted. There is little chance of thermal metamorphism, as the ash should have been thoroughly cooled after transportation several hundred miles in the air. The cavities in which these zeolites have crystallized are not solution cavities, but more probably shrinkage cracks. The index of refraction of the glass and pumice was found to vary between 1.495 and 1.505. The composition of the zeolite is very similar to that of the ash, and it is probably a recrystallization of material derived from the glass and pumice.

In making the analysis of the ash, Professor Phillips found that after being dried at 110°, to remove the absorbed moisture, on standing in the air the ash took up about 3 per cent. of water very readily. This strong hygroscopic character is probably due to the attraction of the capillary tubes of the pumice fragments for water, and may account in part for the resistance to weathering of the ash beds, as described above.

The Clay Beds.

The greater part of the White River formation is not made up of the more interesting types already described, but rather of very fine silt horizontally bedded and color-banded pale pink or brown and pale green. These constitute all of the Oreodon and Titanotherium beds except the nodular layers, channel sandstones, and limestone lenses.

Their minerals are generally so fine grained as to be difficult of determination, but fine angular fragments of quartz, more or less

[11] "Igneous Rocks," Vol. I., pp. 435 on.

weathered feldspars, and mica foils can be detected in all samples, as well as occasional fragments of pumice and volcanic glass. The latter fact shows that showers of volcanic dust were being deposited throughout White River time, but first became the dominant source of sediment at the beginning of the Leptauchenia beds stage. The clay beds represent the normal sheet-flood deposits of a level country and probably are mainly formed of kaolinized feldspars from the granites and pegmatites of the Harney Peak mass, as well as from decomposition products of other minerals less stable in the zone of weathering.

As mentioned above, the profiles developed on the Oreodon clays, as a rule, have steeper slopes and a more angular appearance than those developed on the Titanotherium beds which weather with gentler slopes and more rounded surfaces. The chemical tests suggest that this is due to a difference in the amount of calcareous cement in the two series. The samples of the Oreodon clays averaged 10 to 12 per cent. soluble cement, while the Titanotherium clays averaged only about 4 per cent.

V. Evidence from Shapes of Sand Grains.

Inasmuch as the method of transportation and deposition of sands can often be definitely established by the shapes and average sizes of sand grains, a study of this feature of the White River sands was attempted.

The channel sands, as a rule, have large rounded grains, but the smaller grains are almost all angular and with sharp conchoidal fracture. This is because grains with a diameter less than 0.1 mm. are coated, while submerged, with a film of water which prevents one grain coming into physical contact with another, and thus prevents abrasion. On the other hand, the air forms no such cushion for the smaller grains, and thus wind-rounded sands may show rounded grains down to the finest sands present. The only sample of sand found in the White River which showed thorough rounding even to the finest particles is a fine-grained sandy silt occurring 11 feet above the base of the Leptauchenia beds in the Spring Draw section, Sheep Mountain. This sand has well-rounded grains even in the smallest sizes and contains a notable amount of magnetite and green horn-

blende, minerals which were not abundant in the ordinary channel sands. The garnets contained in the sand are also well rounded. This bed, which is apparently thin and local, evidently represents dune sand deposition. The sands rounded in stream channels have bright surfaces, while those rounded by wind action generally are dull or show etched or pitted surfaces. The sands of the base of the Leptauchenia bed in the Sheep Mountain section, mentioned above, show dull and pitted surfaces, confirming their eolian origin.

Another excellent example of round-grained dune sand of later age than White River is found in the sand grains of the sand-calcite at Rattlesnake Butte, Washington County. The crystals contain about 37 per cent. calcite, crystallizing as perfect scalenohedrons, and contain perfectly rounded sand grains down to the finest present. The quartz approaches spherical shape, the feldspars are generally larger grains and not quite so well rounded. The pink garnets are round. The green hornblende grains are nearly all elliptical in outline, prisms with the corners rounded. Zircon appears as fine yellow grains, about one fourth the size of the average quartzes. Round fragments of volcanic glass have also been found in this sand. The surfaces of the grains have a dull or ground-glass appearance due to wind abrasion.

By a study of part of the White River in eastern Colorado, Matthew[12] came to the conclusion that the White River was largely of eolian origin. A sample of silt from the Oreodon beds of Lewis Creek, Colorado, in the area investigated by Matthew, was examined by the writer and was found to consist largely of fine dust with much volcanic material (pumice and glass) and some of the quartz grains well rounded. It was evidently mainly of eolian origin. Matthew points out that in much of the fossil material from the White River of northeastern Colorado the "hollows in the bones (such as the cellular hollows in the skull . . ., the tympanic bullæ . . ., etc.) are still empty, never having been filled by mud or crushed in."[13] He points out that this could not happen if the bones were fossilized in such a body of water as a lake, as the weight of water and overlying sediment would either fill in the cavity with mud or crush the skull.

[12] Matthew, *Am. Naturalist*, Vol. 33, 1899, pp. 403–408.

[13] Matthew, *Am. Mus. of Nat. Hist. Memoirs*, Vol. I., p. 365.

Thus it seems that while the White River may be mainly of eolian origin in Colorado as Matthew has decided, it was mainly deposited by fluviatile agents in the South Dakota section. The writer's conclusion is that in the sections of the Big Badlands investigated eolian deposition is unimportant, though locally present.

VI. Evidence of Ground Water Circulation.

Though the clay beds of the Badland district seem to be nearly impervious to water, there are abundant evidences of deposition and replacement by the action of ground water. The formation of caliche nodules by deposition of calcareous cementing material with the evaporation of the water at the surface has been referred to.

Throughout the Big Badland district are veins of blue chalcedony in vertical cracks in the clays. These were probably deposited in shrinkage cracks. Other fissures filled with sandy silts or sandstones occur frequently through the Badlands, but most abundantly in the Leptauchenia beds. These filled fissures are often more resistant to weathering than the surrounding clays and stand out in relief as sandstone dikes. Often the sandstone dikes were bordered on each side by chalcedony veins, and in one case there were two or three veins of chalcedony on the same side of the dike, indicating repeated opening of the fissure. The chalcedony veins ordinarily vary in thickness from one quarter inch to three inches and show evidence of gradual filling of the cracks from the two walls. Sometimes the whole vein is silica, but in many cases, specially in the wider veins, well-formed crystals of calcite have formed in the center of the vein, evidently as the last stage of deposition. Sometimes the center of the vein remains open. Occasionally large bell-shaped concretions of chalcedony up to a foot in diameter are formed in the clay. Chalcedony of similar nature is found filling the marrow cavities of fossil bones and the pulp canals of teeth. An interesting case was noted by the writer in a bone in which the lower part of the marrow cavity was filled with fine silt, evidently worked in soon after deposition, and the rest of the cavity was filled with chalcedony. In several cases chalcedony veins were found to cut directly through fossil skulls. Generally no hardening of the clays adjacent to the chalcedony veins is observed, showing that the deposition was from cold water. Some

diffuse cementation of the clays by colloidal silica in the vicinity of chalcedony veins is occasionally noticed, rapidly wearing the edge off the chisel used in the preparation of a specimen.

In many places the basal titanotherium beds contain a series of about 30 feet of clay of blue, lavender, and pink colors when fresh, weathering to a limonite brown and hematite red. This clay is clearly derived from a reworking of the Pierre shale material and even contains Pierre shale fossils, as Baculites, Inoceramus, etc., which are remanie or redeposited as first reported by Loomis.[14] In places where this series is absent, the silts or sands resting directly on the Pierre shale are almost always colored a bright pink color, and on examination the clay particles are seen to be strongly colored by the red oxide of iron.

The uniform presence of these iron-colored beds at the base of the more or less pervious Titanotherium beds and directly above the impervious Pierre shales is evidently a case of iron dissolved out of the White River series and redeposited and concentrated at the limit of downward circulation along the Pierre shale contact.

VII. Conclusions Drawn from the Lithogenetic Evidence.

From the evidence above presented one can draw a fairly good picture of the physiographic conditions at the time of the deposition of the White River beds.

The country was very level, with a gradual slope away from the Black Hills uplift, as evidenced by a slight initial southeast and easterly dip of the beds. The Black Hills were probably still being elevated, but already were sufficiently high and eroded to furnish clastic material from the pre-Cambrian central core.

Fairly sluggish streams meandered across the plain in shallow channels and frequently spread widely out upon the plain when in flood, depositing thin sheets of fine silt. Gradually increasing volcanic action in the Cordilleran region and perhaps in the northern Black Hills furnished a growing supplement to the clastic material from the hills, until in the Leptauchenia stage this source became predominant. It is possible that the statements of Ransome, Schuchert, and others that the Oligocene represents a lull in the uplift and

[14] *Science*, Vol. 19, p. 254.

vulcanism through the Tertiary Cordillera between the periods of storm of the Eocene and Miocene will have to be amended. The presence of a 200-foot bed mainly of volcanic glass and pumice 80 miles away from the nearest possible source of volcanic ejecta, and more likely 200 to 400 miles distant from the eruptive volcanoes, would hardly seem to indicate a period of quiet. Professor Sinclair's description of a considerable thickness of andesitic tuffs and breccias of Titanotherium beds age in the Wind River Basin of Wyoming south of the Bridger-Owl Creek Range, is interesting in this connection.[15]

The only evidence of vegetation found fossilized so far in the Big Badlands are a few hackberry seeds (Celtis), but Hatcher reports the remains of a forest 12 miles north of the mouth of Corn Creek. The plain was here and there dotted with small ponds in which fresh-water algæ and cyprid crustaceans were building up the limestone beds of the series.

Thus we have a general picture of the environment of one of our finest Tertiary mammalian faunas.

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[15] *Bull. Am. Mus. Nat. History*, Vol. 30, 1911, pp. 99–102.

YELLOW FEVER AND FISHES.[1]

By C. H. Eigenmann.

(Read April 22, 1922.)

If it were not for the little fishes, many parts of tropical America would be uninhabitable. This is the excuse I have made, when an excuse was necessary, for devoting all the time I could steal from my family, my students, and my institution to gathering and contemplating the fishes of the rivers and lakes of South America. But pure research no longer needs to apologize, because it has resulted in many cases in unexpected, but lasting, benefits to man.

I was trying to explain the evolution and distribution of the South American fishes to a young manufacturer of veneer fruit boxes. "You must excuse me," he replied, "but that seems like mighty piddling business to me." I have sometimes been inclined to agree with him.

Yellow fever has been prevalent in Panama and Guayaquil, on the coast of Ecuador, almost ever since the places were settled. The French failed in building the Panama Canal on account of the fevers that killed their men. The hospitals were full. It has been reported that to keep ants from crawling up the bed-posts they were set in dishes of water. Later it was found that mosquitoes bred in the dishes of water, and that these mosquitoes carried the fever germs from patients to well persons. The unrestricted breeding of mosquitoes made success impossible.

General Gorgas cleaned out the mosquitoes in Cuba and in Panama. In doing so he not only made the Panama Canal possible, but did far more in demonstrating that the worst pest-holes in the tropics can be made habitable to man of the temperate zone.

Guayaquil was perhaps the worst of all places in South America. Yellow fever always existed and frequently there were outbreaks that closed the port.

[1] Contribution from the Zoölogical Laboratory of Indiana University, No. 193.

General Gorgas was called to Guayaquil and he started a campaign against the mosquitoes. The yellow-fever mosquito is domestic, living in the huts and houses of men. In many places drinking water is stored in rain barrels. In these the mosquitoes breed. Each family rears its own fever mosquitoes. It would be easy to keep mosquitoes from breeding by covering the water with a film of oil. But the native will have none of it. It was found that a fish in each barrel keeps the water free from mosquitoes and thus prevents the spread of yellow fever if a case is accidentally introduced.

Various fishes were tried at Guayaquil and two were finally selected as best. They are the "huaijas" and the "chalacos" of the Guayaquil fishermen. They are abundant and easily obtained in Guayaquil.

Recently I obtained specimens of both sorts and found that they are old friends, long known to naturalists as *Lebiasina bimaculata*, and as *Dormitator latifrons*. The latter, the "chalacos" of the fishermen, is a chuckle-headed fish, of the family Gobiidæ. It reaches a length of over a foot and lives principally in the mouths of rivers in the area affected by the tide, all the way from Guayaquil to California. It is therefore available to exterminate mosquitoes in barrels all along the fever-infested coast of western tropical America. A close relative, *Dormitator maculatus*, lives in the same sort of places in the West Indies and on the Atlantic side of the tropical mainland.

The "huaijas" (*Lebiasina bimaculata*) should in time become even a much more valuable fever eradicator, because it is adjusted to live in much higher altitudes than the chalacos. I have caught it all the way from sea level to 7,000 feet elevation. It can, therefore, be used in the entire fever belt, for yellow-fever mosquitoes do not reach as high as 7,000 feet. I have caught it in great numbers in the most unlikely places. At present it is known to inhabit only the rivers from Lima, Peru, to the region of Guayaquil. It should be possible to introduce it in all the tropical parts of America. Related species extend as far north as Panama.

I became acquainted with it in Lima and later caught it inland from Paita. About Lima it is known as "liza de agua dulce," or sweet-water mullet. It is used as an aquarium fish in Lima and is

found in the branches of the Rimac near the bank of the river opposite the city and probably in all the pools near Lima. I found great quantities in the foul, knee-deep pools and ponds near Puenta Piedra along the railroad between Lima and Ancon, at Chosica, a health resort inland from Lima, and a native caught some very small ones for me at Matucana at an elevation of over 7,000 feet.

At Piura, in northern Peru, where during the dry season the river had been reduced to a few pools, in which the fishes had become concentrated, I caught them in great numbers. A few miles farther south, in the Jequetepeque River, I got them up as high as Llallan. The only drawback to this species is its tendency to jump out of the barrels in which it is placed.

These are not the only fishes that eat mosquito larvæ. In the cisterns of Guayaquil the "millions" are used. The "millions" are minute fishes that came originally from Trinidad or Barbados. The males and females are very different from each other and these fishes give birth to living young. They are called "millions" because if you put a pair of them in a pool "there will soon be millions of them." They are very plentiful in Barbados and the absence of yellow fever from Barbados is credited to the "millions," which do not give the mosquitoes a chance. The "millions" have been transplanted far and wide and are usually part of every novice's aquarium specimens.

In the United States much has been accomplished in eradicating fever mosquitoes with Gambusia, a small fish which eats the larvæ of the mosquito. Mr. S. F. Hildebrand, of the U. S. Bureau of Fisheries, has had charge of this work for the United States Government and has met with very notable success. It seems that there are many fish eradicators of mosquitoes. Different ones are present in different parts of the world. In most places they need some assistance from man to reach the breeding places of the mosquitoes. It is said in Tampico that upon the appearance of a case of yellow fever the Standard Oil Company spent $3,000 per week to keep the waters oiled to suppress the mosquitoes until a small fish living in the very neighborhood of the plant was put to work and saved all of that expense.

Of all of the fishes, the "liza de agua dulce" of Lima, or the "huaijas," as it is called at Guayaquil, offers the greatest usefulness.

It can easily be caught, it can easily be transported, and will live at any altitude in which the fever mosquitoes are likely to be found.

With the chalacos available to control the mosquitoes along the coast and the huaijas to control them from the coast to several thousand feet, the means are at hand to entirely eradicate fevers from the Pacific slopes of Tropical America.

With the above paper, portions of a letter dated March 27, 1922, were read, from Dr. Henry Hanson, Director General of the "Compañia Sanitaria Contra la Fiebre Amarilla en el Perú." Dr. Hanson reported that he was sending specimens of six species of fishes with which experiments were made in mosquito control. He says in part:

> We found two to be very good larvæ destroyers. . . . I think our campaign has demonstrated that using fish is the only rapid method of handling a yellow fever epidemic.

Dr. Hanson further reports that only two of the six species tried proved valuable. They are the fishes known locally as "chalquoque" and "life," pronounced lē-fā.

Addenda.

The "chalquoque" is *Lebiasina bimaculata* C. & V., called "liza de agua dulce" in Lima, and is the "huaijas" so successfully used for mosquito work in Guayaquil. It is found everywhere between Lima and Guayaquil, and in the Rio Rimac it is found from sea level to Matucana at over 7,000 feet.

Another member of the genus is found in the Atrato and San Juan basins in Colombia. Species of a very closely related genus, *Piabucina*, are found from Guayaquil north to the Chagres. The species will be considered, several of them figured in a volume on the fishes of northwestern South America now going through the press (*Mem. Carnegie Mus.*, IX., 1922). The species are members of the *Lebiasininæ* of the Characidæ.

The "life" is *Pygidium punctulatum piuræ* E., recently described by myself from Piura. It is much of a surprise that this species proves to be a mosquito larva eater. It is, as far as known, restricted to northwestern Peru, but a close relative, *P. punctulatum* (C. & V.) is found in the Rio Rimac from the ocean to several thousand feet at least. It is the "bagre" of the Rio Rimac. The

name "bagre" is used almost everywhere in Latin America, but is applied to a great variety of different catfish-like fishes. If the "life" of northern Peru shares its habit of eating mosquito larvæ with the rest of the members of the genus Pygidium, it is a most important discovery. Various species of Pygidium are found in all the mountain streams of South America and a few are found in the hot lowlands of the Amazon Valley. It belongs to a peculiar family of South American catfishes which are called "bagre," "bagrecitos," "capitan," etc. I monographed the family. ("The Pygidiidæ, a family of South American Catfishes," *Mem. Carnegie Mus.*, VII., No. 5, pp. 259–398, Plates XXXVI.–LVI., 1918.)

In Arequipa I caught specimens of another species of "life," *Pygidium quechuorum*, in such numbers that a devout passer-by exclaimed, "It exceeds the miraculous draft of St. Peter!"

The underlying structure of most of the members of the family is the presence of spines on the opercle and interopercle and the presence of two barbels at the end of the maxillary where other catfishes carry but one barbel. The underlying habit is their eel-like movements and their ability to hold an advance, once gained, by means of the spines on the head. They get under and between rocks, eel their way into holes, and can climb vertical walls.

The queerest members of the family live as commensals or parasites in the gill cavities of larger fishes, and some of them have gained an evil reputation and struck terror into the natives of the entire hot country by the reputed habit of entering the urethra of bathers, sometimes requiring operations or causing death.

Of the genus *Pygidium* about 70 species are known. They range from a few millimeters to 390 mm. in length and are found from sea level to at least 12,000 feet. They are abundant in and about Lake Titicaca. I caught them in southern Chili to northern Colombia and Guiana. They are also abundant in the mountain streams of southeastern Brazil. The other four species of fishes tried in northern Peru and sent by Dr. Hanson are:

1. The "bagre." This is *Pimelodella yuncensis* Steindachner confined to the rivers between Pacasmayo and Paita. It may be found as far south as the Rio Santa. It is not found in the Rimac.

It may be found a little farther north than Paita, but it is not found as far north as Guayaquil, where its place is taken by a related species, *Pimelodella elongata* (G.). The genus *Pimelodella* is very widely distributed in South America "from Buenos Aires to Guiana and Venezuela to the base of the Andes; west of the Andes from Peru to the Chagres River to Panama." I published a monograph of the genus ("*Pimelodella and Typhlobagrus*," *Mem. Carnegie Mus., Pittsburgh*, VII., No. 4, pp. 229–258, Plates XXI.–XXXV., 1917) with figures of most of the species. The Pimelodellas are small, long-whiskered catfishes and not much is to be expected of their performance as mosquito eradicators.

2. The "tripon" is *Curimatus peruanus* E., a species recently discovered by me at Sullana. As far as known, it occurs only in the Chira River and at Chiclayo, where the yellow-fever commission caught it. I did not get it at Pacasmayo, only a few miles farther south. There are two other species of Curimatus in Guayaquil and five more in Colombia.

The genus is very widely distributed in the hot lowlands east of the Andes. The species do not have teeth and nothing is to be expected of them as mosquito larva eaters. They seem to feed on slime. There are 50 or more known species. I reviewed the group of the toothless Characins to which the genus belongs in 1889 (*Annals N. Y. Acad. Sci.*, IV., pp. 1–32, 1889).

3. The "cachuelo" is *Bryconamericus peruanus* (M. & T.), found in all the streams between the Rimac and the Esmeraldas in Ecuador. In the Rimac it occurs from sea level to over 7,000 feet elevation. It should prove a valuable larva eater. A second species of the genus is found at Guayaquil. North of Guayaquil, in Colombia, several other species are abundant. The genus is widely distributed east of the Andes. It is a member of the Tetragonopterinæ of the Characidæ. Among the Tetragonopterinæ there should be many species available for mosquito work.

I found insect remains in the intestines of many of the species, some of which seem to specialize in the insects naturally blown into the river. The entire group of the Tetragonopterinæ has been monographed. Most of the species have been figured. Three parts of the

monograph have been issued, the fourth is in press (*Mem. Mus. Comp. Zoölogy*, XLVII., Cambridge, Mass., 1917, 1918, 1921).

4. The "mojarra" is *Aequidens rivulatus* (G.) of the Cichlidæ. The family is abundant from Texas south. There are seventeen different species of the family between Pacasmayo and Panama. Only the present species is found in Peru. I found it very abundant in pools in the river bed at Piura and in the Jequetepeque River. It is a very active fish with the habits and general appearance of some of our North American sunfishes.

Additional specimens of fishes used in Guayaquil in yellow-fever work were received early in June, 1922, from Dr. W. Pareja, Director de Sanidad, Guayaquil.

Part of my letter of June 5, 1922, to Dr. Pareja follows: "I hasten to inform you that the 'millones' are *Acanthophacelus reticulatus* (Peters). These little fishes are native in Barbadoes, Trinidad, and along the coast streams of Guiana and Venezuela. They have been widely distributed for mosquito work.

"The 'chatas' are *Astyanax festæ* (Boulenger). This fish has only been taken in the Chone, Portoviejo and the Guayas basins, all in Ecuador.

"The 'brejas' belong to two species: (*a*) *Curimatus troscheli* G. is a lowland fish found only in the Guayas basin and reaches a length of 203 mm. (*b*) *Prochilodus humeralis* G. is similarly confined to the Guayas and reaches 280 mm. Other species of the genus Prochilodus called 'boca chica' are found in countless millions in the Atrato and Magdalena rivers of Colombia and all through the east from the ocean to 3,000 feet. They grow to a considerable size (390 mm.) and are dried and sold to the laborers of Colombia for food."

This account may well be closed with an extract from a letter received from Dr. Hanson, mentioned above. It is dated June 9, 1922:

"It appears that the fact that the 'life' is an effective larvæ consumer is well established by the fact that in the Province of Santa we did nothing except distribute fish in all containers, and did not attempt to throw out or filter any of the water which contained great

numbers of Stegomyia larvæ. We used the life almost to the exclusion of other fish because it is hardier and does not have the jumping tendencies of the chalquoque and other fish. The other fish which we sent up were rather too delicate to suit the conditions with which we had to contend. We found many of them dead on reëxamining the container some days after the first distribution. This did not occur with the life where it had any reasonable care.

"We secured more than 1,000,000 of these fish, 80 per cent. of which were lifes. We have records of the distribution of 857,561 fish.

"Fortunately we feel certain that we have the question of yellow fever completely dominated in Peru and we believe on the entire Pacific coast of South America."

Zoölogical Laboratory,
University of Indiana.

ARC SPECTRA AND IONIZATION POTENTIALS IN DISSOCIATED GASES.

By K. T. COMPTON.

(*Read April 21, 1922*)

The great complexity of spectra is due, in part, to the fact that the molecules may exist in various states of association, dissociation, and ionization, each type of molecule or atom giving rise to its own characteristic spectrum. A discovery of the exact state of the atom or molecule giving rise to each part of the spectrum of a substance is of great importance as regards both the theory of spectral emission and the theory of atomic and molecular structure. At the Palmer Physical Laboratory this problem is being attacked from three different angles. This paper presents some discoveries made by two of these methods and discusses their significance.

Hydrogen.

Bohr's theory is believed satisfactorily to account for the known properties of hydrogen atoms. Hydrogen ordinarily exists, however, in the form of diatomic molecules, whose properties have not been adequately explained by any hypothesis of molecular structure yet proposed. Those properties of the hydrogen atom which Bohr's theory explains are the series spectrum and the energy required to produce radiation or to ionize the atoms, commonly expressed as radiating and ionizing potentials, respectively. Other radiating and ionizing potentials and another type of spectrum are believed to be due to hydrogen molecules. In no case has there been direct and definite evidence as to which type of spectrum is due to the atom and which to the molecule, although sufficient indirect evidence is at hand to ascribe the series spectrum to the atom and the secondary spectrum to the molecule with considerable certainty. There has been no experimental evidence at all as to which entity to ascribe each radiating or ionizing potential observed in hydrogen. The assignment of

particular critical potentials to atom or molecule has been justified by the degree of consistency with probable processes of dissociation, radiation, or ionization. The present investigations have yielded definite and direct experimental evidence on the above points and have thrown new light on the nature of low voltage arcs and on the manner of excitation of the spectrum.

Low Voltage Arc in Hydrogen.

Dr. O. S. Duffendack has studied the relation of voltage to arc currents and spectral excitation. The arcs were produced in gas at pressures between 0.5 and 5.0 mm. between two electrodes, as shown in Fig. 1. *AB* was a tube of thin tungsten foil, which could be

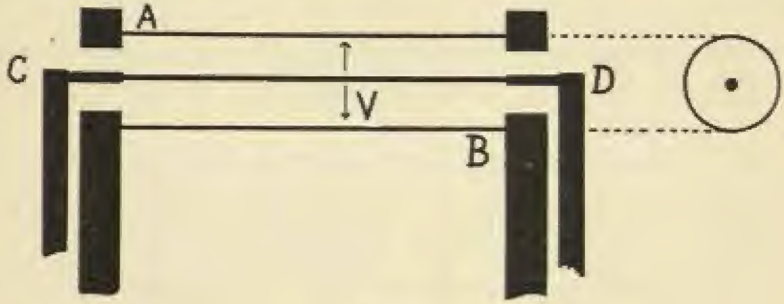

FIG. 1.

heated by a current through water-cooled leads. *CD* was a 20-mil tungsten wire passing axially through the tube and also electrically heated. The potential drops across the tube and the filament could be adjusted to practical equality, so that they behaved as equipotential electrodes to electrons emitted from the wire and drawn to the tube by an applied difference of potential V. The tube was 30 mm. long and of 3.7 mm. radius. Ionization of gas within the tube was detected by increase of current I between the electrodes, and the spectrum of the excited radiation was observed through an open end of the tube.

With the filament heated to a bright incandescence, but with the tube relatively cool, the current-voltage variation is illustrated by Fig. 2. From curve (a) it is seen that the current first is very small and increases slowly with voltage until the ionization potential, about

16.3 volts, is approached. Ionization is first detected a couple of volts below this because the electrons are emitted from the filament with small initial speeds. When the current has increased to a certain value, the arc strikes, with a sudden large increase in current, at a voltage higher than the minimum ionizing potential. As the voltage is then diminished the arc persists down to the minimum ionizing potential, but breaks at this voltage. If the filament is hotter, so as to emit more electrons, as in curve (b), the arc strikes at a voltage

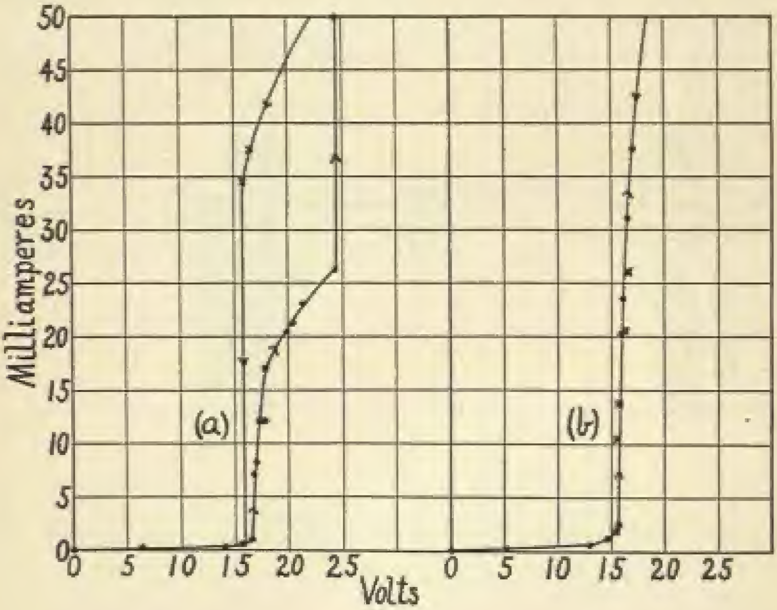

FIG. 2.

nearer the ionizing potential and still breaks at this voltage. In no case was it possible to produce or maintain the arc at a voltage less than the minimum ionizing potential, about 16.3, except for a very small amount depending on the temperature of the filament, due to the initial energy of emission, and easily allowed for.

When the outer tube was very hot, the conditions were quite different, as seen by Fig. 3. The initial current is larger, there is ionization beginning at about 10 volts, and stronger ionization at about 14 volts. In curve (a) the arc struck and was maintained at about 13.8

volts, while in curve (*b*), with the tube still hotter, the arc struck at about 10.0 volts, and there was no indication of further ionization near either 14 or 16 volts.

The essential difference between these two cases lies in the fact that in the latter cases the tube was sufficiently hot to completely dissociate the hydrogen inside it into atomic hydrogen. Thus the critical potential 16.3 is the ionization potential of hydrogen *molecules*, whereas 13.6 volts is that of hydrogen *atoms*. The critical potential, 10.1 volts, is also due to *atomic* hydrogen and is shown later to be

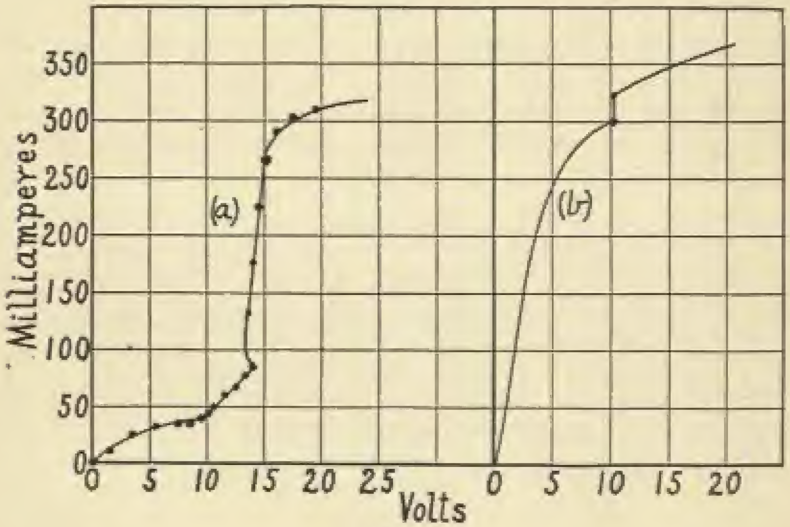

FIG. 3.

due to excitation of radiation from the atoms. That this radiation results in ionization is probably due to a "cumulative" effect—atoms being struck by electrons while they are in a partially ionized state because of absorption of energy of radiation from neighboring atoms.

The above experiments do not fix the values of the critical potentials as accurately as do the experiments by the following method, but they do determine beyond all question the sources of the observed effects. *These are the first laboratory experiments to be performed in an atmosphere of atomic hydrogen.*

Incidentally the character of the current-voltage curves yields knowledge of the processes which produce the arc. At voltages so low as to avoid ionization the current is due entirely to the electronic emission from the filament, and this is limited to a very small value, independent of the temperature of the filament, provided it is high, by the negative space charge of the electrons immediately surrounding it.[1] When ionization occurs each positive ion, drawn toward the filament, neutralizes the space charge of about 243 electrons and thus permits the escape of that additional number. This large number of electrons liberated by each positive ion is due to the relatively small speed with which the heavy positive ion moves through the region of space charge. The current increase is therefore due not so much to the addition of ions by ionization as to the effect of the positive ions in permitting the escape of many more electrons from the filament. Thus the current increases with increasing voltage until it approaches the thermionic saturation current characteristic of the size and temperament of the filament. Then there are no longer sufficient electrons to neutralize the effect of the positive ions near the filament so that the space charge changes from negative to positive, creating an accelerating field for the emitted electrons and giving the saturation thermionic current plus the ionization current. This is the arc. It is characterized by its sudden appearance and by the concentration of luminosity in the region very near the filament, where the principal portion of the potential drop occurs, with positive space charge.

Excitation of the Hydrogen Spectrum.

In molecular hydrogen there was no visible spectrum until the arc flashed in at or above 16.3 volts. Then there appeared the Balmer series lines and part of the secondary, or band, spectrum. The part appearing was Group I., of Fulcher's Classification,[2] which includes those lines which show little intensification with increasing voltage. These lines are also those which show no Zeeman effect and are apparently the ones which Merton found not to be enhanced by the admixture of helium with the hydrogen.[3]

[1] Langmuir, *Phys. Rev.*, 2, p. 543, 1913.

[2] *Astrophys. Jour.*, 37, p. 65, 1913.

[3] *Roy. Soc. Proc.*, *A.*, 96, p. 382, 1919.

In atomic hydrogen the secondary spectrum was entirely absent, but the series spectrum appeared strongly when the arc struck at 13.5 volts and could be detected down to 10.1 volts.

These results confirm the prevailing opinion that the series spectrum is due to atoms and the secondary spectrum is due to molecules. The voltages at which the spectrum appears in atomic hydrogen are exactly those to be expected from Bohr's interpretation of the series formula. The exciting voltage in molecular hydrogen is that to be expected if the critical 16.3 voltage is interpreted as dissociation of the molecule plus ionization of one of the atoms as the result of single electron impacts, giving $16.3 - 13.5 = 2.8$ volts as the heat of dissociation in equivalent volts. This is almost exactly the value given by Bohr's theory, but is lower than the value 4.06 volts calculated from Langmuir's measurement of the heat of dissociation of hydrogen.[4]

The interpretation of the secondary spectrum is puzzling. It is certainly due to molecular hydrogen, but the nature of the emitting molecule is uncertain. It is probably not due to neutral H_2 molecules, since these have no absorption in the visible spectrum. $\overset{+}{H}_2$ molecules are known to be present in fairly large concentration in an arc, but more in a high voltage discharge in a large vessel at low pressures. H_3 molecules are also known to be present. Possibly one of these may give rise to Fulcher's Group I. and the other to his Group II., but evidence on this point is rather conflicting. An observation of possible interest in this connection is that we found the secondary spectrum to disappear, when the outer tube was heated, at temperatures certainly too low to have dissociated H_2 molecules to any great extent. This suggests that the molecule responsible for the observed secondary spectrum lines is less stable than H_2. As far as this is concerned, it may be either $\overset{+}{H}_2$ or H_3. The absence of a Doppler shift for these lines may possibly point to the neutral H_3 molecule as the agent. In this connection it may be mentioned that R. W. Wood has very recently succeeded in drawing off pure atomic hydrogen from the center of a long Geissler tube, where only the series spectrum is visible, and he finds that the presence of a tungsten

[4] *Am. Chem. Soc. Jour.*, 37, p. 417, 1915.

wire in this stream causes the atoms to recombine at its surface, with the emission of the secondary spectrum and a heating of the wire.*

Further Critical Potentials in Hydrogen.

The problem has been attacked from another angle by Dr. P. S. Olmstead, who used a modification of the Lenard method of investigating ionization potentials, illustrated in Fig. 4. Electrons from the

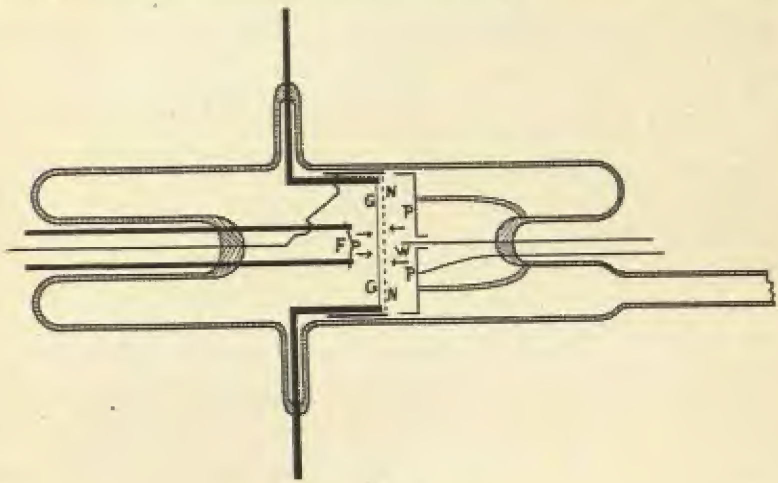

FIG. 4.

hot central portion of a tungsten filament F were drawn toward a wire net NN through an accelerating difference of potential V. Those which passed the net NN encountered a retarding field V_r sufficient to stop them.

The two distinctive features of the apparatus were the system of detecting electrodes P and W and a grid GG of twenty parallel tungsten wires. PP was a platinum plate and W was a fine platinum wire extending but a short distance in front of the plate PP. When the electrons from F collided with and produced radiation from atoms or molecules near the net NN a considerable amount of this radiation fell on the plate PP, whereas the wire W received only a negligible amount of radiation because of its very small area. Either PP or W

* Since this paper was written very decisive evidence on these points has been obtained and is to be published by Dr. Duffendack in the Astrophys. Jour.

could be connected to the quadrant electrometer, which measured both the current due to ionization of the gas between *NN* and *PP* and the photoelectric current from *PP* or *W* due to the radiation set up by electron bombardment of the gas. Those effects, setting in at critical potentials which were due to radiation, were relatively more pronounced when the plate *PP* was joined to the electrometer. Effects of ionization were relatively more marked when the wire *W* was used, since this wire could collect all the positive ions formed, but would receive but little of the radiation.

The grid *GG* was used to vary the proportion of atomic hydrogen in the neighborhood of the net *NN*. This was done by heating it to a high temperature by an electric current, if atomic hydrogen was desired. The relative proportion of atomic to molecular hydrogen was never large, but was sufficient for our purpose. Those critical potentials which were relatively more marked when the grid *GG* was hot were ascribed to atomic hydrogen and the others to molecular hydrogen.

By thus varying the sensitiveness to detection of radiation, and by varying the amount of atomic hydrogen present, it was possible definitely to determine the nature and origin of the effect setting in at each of the critical potentials of hydrogen.

The critical potentials are shown by Fig. 5, taken with the plate *PP* joined to the electrometer, and with the grid *GG* hot. Similar curves with the wire *W*, or with the grid cold, show fewer "breaks," as expected. The interpretation of these critical potentials is indicated by the "ratio" curves of Fig. 6. These curves show ratios of electrometer deflections under the various conditions. I (on)/I (off) means the ratio of the deflections using the wire *W* (relatively sensitive to effects of ionization) with the grid current on (hot) and off (cold) respectively. R (on)/I (on) means the ratio of the deflections with the plate (relatively sensitive to radiation) to those with the wire (relatively sensitive to ionization), the grid current being on in both cases. When these curves are interpreted in the light of the discussion above, and when proper allowance is made for the effect of the initial velocities with which electrons are emitted from the filament (which introduces a small correction for weak effects and a

larger one for strong effects), the interpretation of each of the critical potentials is as shown in Table I. Setting in of new effects produces a change in curvature of the curves of Fig. 6. Such changes do not necessarily indicate new effects, however, and the effects may not be shown prominently at exactly the same voltage as in Fig. 5. Some caution is needed, therefore, in interpreting the curves in cases which

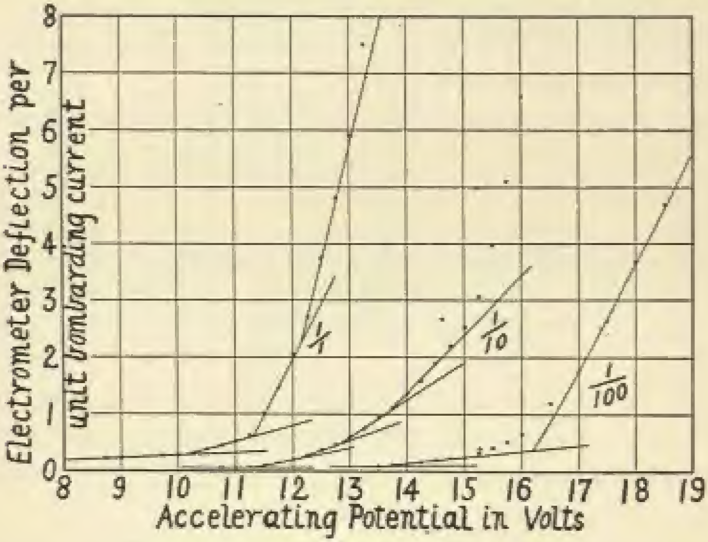

FIG. 5.

correspond to weak effects. We believe, however, that a consideration of the full set of curves points unmistakably to the following results:

TABLE I.

Critical Potential.	Nature.	Due to	Process.
10.1 volts....	Radiation	Atom	Direct
11.3........	Ionization	Molecule	Ionization without dissociation
12.1........	Radiation	Atom	Direct
12.8........	Radiation	Molecule	Dissociation plus radiation from an atom
13.6........	Ionization	Atom	Direct
16.2........	Ionization	Molecule	Dissociation plus ionization of an atom

As an example of the method of interpretation, consider the 13.6 volt effect. The R (on)/I (on) curve indicates that it is due to ionization. The I (on)/I (off) curve indicates that it is due to the

but do not give such decisive information. The above critical poten-
atom. The other two curves are consistent with these interpretations,

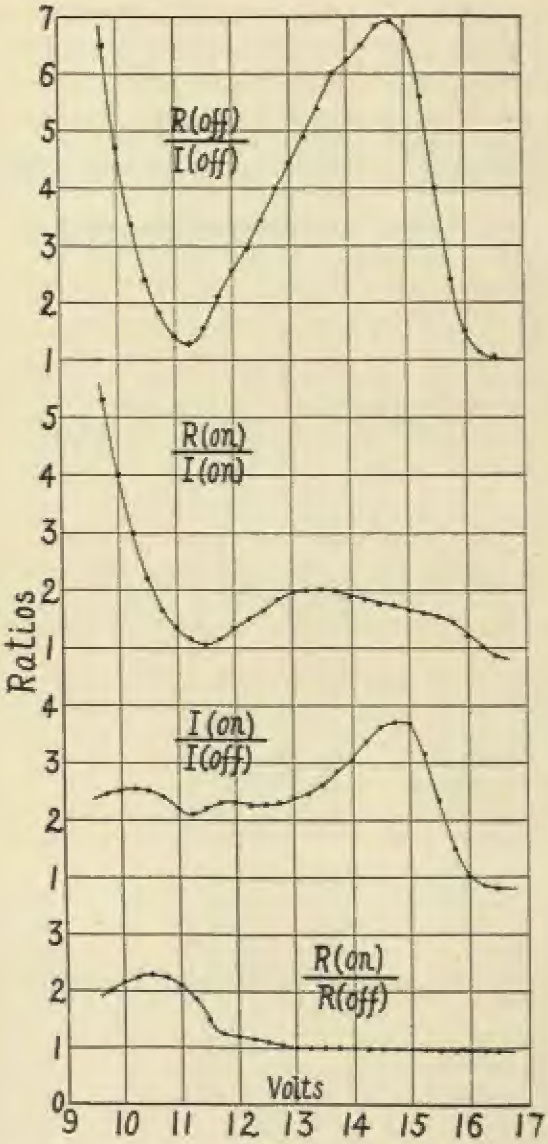

Fig. 6.

tials are determined experimentally to within probably 0.2 volt, when the corrections for velocity distribution are considered.[5]

The three atomic effects correspond exactly, by the quantum relation $eV = h\nu$ to the first two lines 1216 Å. and 1026 Å. and to the convergence wave length 911 Å. of the Lyman series. This is, I believe, the first time that evidence has been obtained for the excitation of the second member of the series separately. We have some evidence of the separate excitation of the third member, also, and are setting up a new apparatus designed to detect atomic radiation effects very sensitively, with entire freedom from effects of ionization. With this we expect to make a more thorough investigation of the excitation of the successive members of the Lyman series.

The 11.3 volt molecular effect is the formation of $\overset{+}{H}_2$, an ion which is found by positive ray analysis to be very abundant, although Bohr's theory of the molecule represents it as unstable. The 12.8 and 16.2 volt effects correspond to the processes $H_2 = H + H_r$ and $H_2 = H + \overset{+}{H}$, respectively. The heat of dissociation is given, in equivalent volts, by $12.8 - 10.1 = 2.7$ volts or by $16.2 - 13.6 = 2.6$ volts. This is in good agreement with the results of the preceding method, but smaller than Langmuir's value.

The critical potentials reported by previous observers are given in Table II. The considerable lack of consistency among the various

TABLE II.

Experimenter	Radiation			Ionization			
Davis and Goucher[6]	11.0,		13.6,	11.0,		15.8	
Mohler and Foote[7]	10.4,	12.2,			13.3,	16.5	
Foote, Mohler and Kurth[8]	10.5,	11.8,				16.0	
Horton and Davies[9]	10.5,		13.9,		14.4,	16.9	
Franck, Knipping and Krüger[10]			13.6,	11.5,		17.1,	30.45
Compton and Olmstead[11]	<10.8,		13.4,	>10.8,		>15.8	
Boucher[12]	10.1,				13.6,	15.6	
This Investigation	10.1,	12.1,	12.8,	11.3,	13.6,	16.2	

[5] Smyth, *Phys. Rev.*, 14, p. 409, 1919; Compton and Olmstead, *Phys. Rev.*, 17, p. 52, 1921.

[6] *Phys. Rev.*, 10, p. 101, 1917.

[7] *Jour. Optical Soc. of Am.*, 4, p. 49, 1920.

[8] *Phys. Rev.*, 19, p. 414, 1922.

[9] *Roy. Soc. Proc.*, *A*, 97, p. 23, 1920.

[10] *Ber. d. D. Phys. Ges.*, 21, p. 728, 1920.

[11] *Phys. Rev.*, 17, p. 45, 1921.

[12] *Phys. Rev.*, 19, p. 189, 1922.

critical potentials reported is probably due to (1) the fact that the velocity distribution correction is likely to be exaggerated for the stronger effects, unless a systematic method such as was suggested by Smyth or its graphical equivalent is used, (2) two effects occurring close together are likely to be reported as one, (3) the form of apparatus may be such as to enhance ionization effects relatively to radiation effects, or vice-versa.

Nitrogen.

This gas has been studied by Dr. Duffendack, using the low voltage arc method as in the case of hydrogen. It was found that the gas could not be appreciably dissociated into atomic nitrogen by any attainable temperature of the tungsten tube furnace, although it was possible to produce atomic nitrogen (Strett's "active" nitrogen)[13] by electronic bombardment at lower current densities and voltages when the tube was very hot than when it was cooler.

Low Voltage Arc in Nitrogen.

Typical current-voltage curves are shown in Fig. 7. In curve

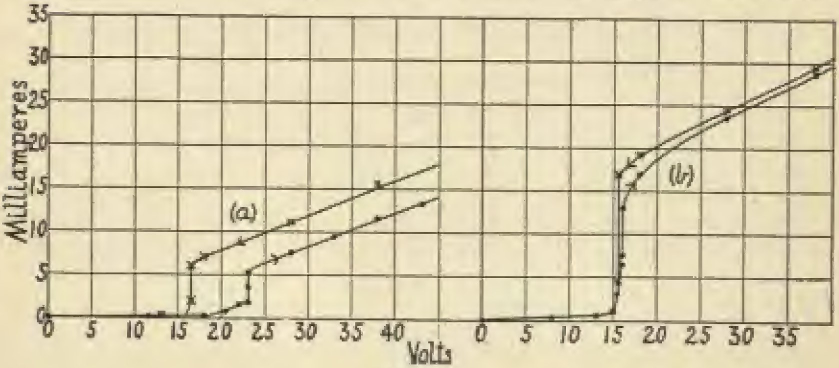

Fig. 7.

(*b*) the filament was hotter and the gas pressure lower than in curve (*a*), both conditions favoring the production of the arc. The "break" point of the arc was found to be 16.15 volts as an average of a large number of closely agreeing observations made under various experimental conditions. Under no conditions could the arc be

[13] *Roy. Soc. Proc.*, *A*, 85, p. 219, 1911, et seq.

maintained at a voltage lower than this, which is the minimum ionizing potential of the molecule.

The shape of these curves differs from that of the hydrogen arc curves in that the current rapidly increases with increasing voltage above the arcing voltage. This is probably due to the greater complexity and stability of the molecule, since it seems to be related to the rapid increase of intensity of the negative band spectrum as the voltage is increased. This negative band spectrum is due, as shown by its behavior in a positive ray apparatus and its relation to exciting voltage, to excitation of the positively charged molecules $\overset{+}{N}_2$, which can evidently be partially or completely ionized without breaking up into atoms.

In the neighborhood of 70 volts the arc current began to increase rapidly again, a brilliant "flare" setting in and the current becoming as large as 15 amperes. Simultaneously with this increase the lines 5006 and 5003 of the nitrogen line spectrum appeared. At about 90 volts the lines 5680 and 5667 were visible. If the filament is hot, this "flare" could be maintained at voltages as low as 25 volts, after setting in at 70 volts, but no lines of the line spectrum were seen below 70 volts. Lines of the tungsten spectrum were always observed in the flare.

Comparison with observations by Strutt[14] shows that this flare was due to the presence of *atomic* nitrogen, and that the presence of the tungsten spectrum was due to the burning of tungsten in atomic nitrogen. It is significant that the flare could be produced at 40 volts, instead of 70 volts, if the outer tungsten tube were very hot. This is the only evidence of dissociation of nitrogen by the hot tube. It is also of interest to note that Langmuir discovered an anomalous behavior of thermionic emission from tungsten filaments in nitrogen at low pressures and voltages above 70 volts, and attributed this to a chemical action which did not occur at lower voltages.[15] It is not easy to explain why the formation of atomic nitrogen should increase the current through the arc so enormously. Multiple ionization, ionization by chemical action on tungsten, or heat developed by atomic recombination at the surface of the filament may be important factors.

[14] *Loc. cit.*

[15] *Phys. Rev.*, 2, p. 450, 1913.

Excitation of the Nitrogen Spectrum.

The nitrogen spectrum consists of three groups of positive bands, two of which appear in the visible spectrum and one only in the ultra-violet, a group of negative bands and a line spectrum which has not been resolved into series.

The first group of positive bands was first seen when the arc struck and increased slightly in intensity as the voltage was raised. The second group was first seen when the arc struck and decreased in intensity with increasing voltage. L. and E. Bloch[16] and Brandt[17] have detected positive bands at voltages as low as 12 and 7.5 volts, respectively. It is therefore evident that the positive bands are due to excitation of neutral molecules.

The negative bands were not observed until the voltage was a volt or two above the minimum ionizing potential, and increased markedly in intensity with increasing voltage. Rau[18] has found a Doppler shift in the negative bands in positive rays. These and other facts indicate that the negative bands are due to excitation of positively charged molecules $\overset{+}{N}_2$.

It is surprising that as high a voltage as 70 volts was required to excite the line spectrum, and that only the lines 5006, 5003, 5680, and 5667 were observed. There are a number of other lines usually listed as at least as intense as these of which no trace was found. It seems probable, as is suggested by the magnitude of the Doppler shifts[19] for the nitrogen lines, that the lines which we observed are enhanced lines, and the remaining strong lines in the visible spectrum may correspond to still higher ionization. The Doppler shifts suggest that a line 3995 Å. may be an unenhanced line. Unfortunately we did not investigate this region of the spectrum, but we plan to make a photographic test in the near future. At any rate, it seems certain that the simpler parts of the nitrogen line spectrum lie in a region of wave-lengths far removed from the visible, and that the difficulty of dissociating the molecules makes it difficult to excite the line spectrum under conditions which can easily be interpreted.

[16] *Comptes Rendus*, 170, p. 1380, 1920.

[17] *Zeit. f. Phys.*, 8, p. 32, 1921.

[18] "Vortrag auf der Physikertagung in Jena," 1921.

[19] J. J. Thomson, "Positive Rays."

SUMMARY.

These investigations open up new methods for the study of conditions of dissociation, ionization, and excitation of radiation of multiatomic molecules. They have given a definite interpretation to the critical potentials of hydrogen and yielded valuable information regarding the processes of ionization and radiation in nitrogen. Similar work with iodine is in progress.

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MANNA, NECTAR, AND AMBROSIA.

By PAUL HAUPT.

(Read April 22, 1922.)

The Biblical manna, which the ancestors of the Jews are said to have eaten for forty years until they came to the borders of Canaan, is not the manna of commerce, which is a saccharine exudation obtained in Sicilian plantations, during July and August, by making transverse incisions through the bark of flowering-ash trees (*Fraxinus ornus*). This is employed as a gentle laxative for children and is still largely consumed in South America. The Jews' manna is generally supposed to be the honey-like exudation of a species of tamarisk on the Sinaitic peninsula. The flow of manna from the soft twigs of the *tamarix Gallica,* which is due to their being punctured by a scale insect, appears only during certain months (about the end of May and in June). It could not have yielded the daily provision of more than 300 tons;[1] the annual quantity produced on the Sinaitic peninsula is only 500 or 600 lbs. Nor could it have been ground in querns, or pounded in mortars, and baked[2] in baking-pots.[3] It has the consistency of wax in the early morning, but melts in the heat of the sun (Exod. 16, 21). This Sinaitic manna is still collected by the Arabs and sold to the monks of St. Catherine on Mount Sinai, who supply it to the pilgrims and tourists visiting the convent.

I showed in my paper *The Burning Bush and the Origin of Judaism,* which I presented at our General Meeting in 1909, that the mountain whence the Law is said to have been given to Moses can not have been situated on the Sinaitic peninsula; it must have been a

[1] They are said to have collected an *omer* per day per person (Exod. 16, 16. 36). An omer is nearly a gallon (more accurately, 3.644 liters). According to Exod. 12, 37 (*cf.* 38, 26; Num. 1, 46; 26, 51) there were more than 600,-000 men not including Levites, women and children; so there would have been more than two million people. These numbers are, of course, impossible; see Gray, *Numbers* (ICC) p. 12; contrast EB[11] 25, 139[b], below.

[2] For. Heb. *biššél,* to bake, *cf.* 2 S 13, 18; AJSL 26, 16; ZDMG 63, 517.

[3] See the cut on p. 64 of the translation of *Leviticus* in the Polychrome Bible; *cf.* MLN 38, 433; also ZDMG 61, 714, l. 10; JBL 36, 256.

volcano in northwestern Arabia (PAPS 48, 355).[4] The name *Sinai* is derived from the Assyrian name of the moon-god, *Šin*. About four days' journey S.E. of Tebûk in northwestern Arabia there is an isolated table-mountain of sandstone with a high, pitch-black extinct volcano on its flattened summit, which is called *al-Badr*, i.e. the Arabic word for *full moon*. At the foot of the northern side of this sacred mountain (which was visited, on July 2, 1910, by Professor Musil, of Vienna, who will lecture in this country next fall) there are twelve large blocks of sandstone, known as *al-madâbiḥ* = Heb. *mizbĕḥôṯ*, sacrificial altars. Similar blocks are found at the western end. On the southern side there are The Caves of the Servants of Moses, Arab. *Maḡâ'ir 'abîd Mûsâ*. The ancestors of the Jews seem to have proceeded from Elath, at the northeastern end of the Red Sea, in a southeastern direction (JAOS 34, 526; 35, 387-390).

Forty years ago the distinguished mythologist W. H. Roscher published a monograph[5] advancing the theory that nectar and ambrosia were kinds of honey like the Biblical manna. We call the saccharine fluid excreted by flowers, which attracts insects or birds, *nectar*, and we apply the name *ambrosia* to the food of certain wood-boring beetles, which consists of certain minute hyphomycetous fungi coating the walls of their galleries. In the Homeric poems (in which eighth-century Ionians describe twelfth-century events; *cf.* EB[11] 8, 426[b]) *nectar* is the drink, and *ambrosia* the food of the gods; but in the Doric fragments of Alcman (the greatest lyric poet of Sparta,

[4] Note the following abbreviations:—AJSL=*American Journal of Semitic Languages;*—AV=Authorized Version;—BA=Delitzsch and Haupt, *Beiträge zur Assyriologie;*—BL=Haupt, *Biblische Liebeslieder* (Leipsic, 1907); —CD=Century Dictionary;—EB=Cheyne-Black, *Encyclopædia Biblica;*—EB[11]=*Encyclopædia Britannica*, 11[th] edition;—*Est.*=Haupt, *The Book of Esther* (Chicago, 1908);—GB=Gesenius-Buhl, *Hebräisches Handwörterbuch;* —GK=Gesenius-Kautzsch, *Hebräische Grammatik;*—ICC=International Critical Commentary;—JAOS=*Journal of the American Oriental Society;*—JBL =*Journal of Biblical Literature;*—JHUC=*Johns Hopkins University Circular;*—MLN=*Modern Language Notes;*—OD=*New English Dictionary*, Oxford;—OT=Old Testament;—PAPS=*Proceedings of the American Philosophical Society;*—RV=Revised Version;—S=Samuel; SEP=*Saturday Evening Post;*—VS=Brockelmann, *Vergleichende Grammatik der semitischen Sprachen*, vol. 2 (Berlin, 1913);—ZDMG=*Zeitschrift der Deutschen Morgenländischen Gesellschaft.*

[5] W. H. Roscher, *Nektar und Ambrosia*, Leipsic, 1883.

about 650) *nectar* is the food, and in Sappho (who flourished about 600, and who shared with Alcæus the supremacy of the Æolian school of lyric poetry) *ambrosia* is the drink.

It would seem, however, that both nectar and ambrosia denote *fragrant fat*, especially the nidorous smell of the sacrifices ascending to heaven. The fragrant steam arising from a burning sacrifice was the nourishment of the gods. Ethereal beings feed on vapors, not on solid meats.[a] The Old Testament says that an offering made by fire yields a sweet savor to JHVH. For *Let the Lord accept an offering* (1 S 26, 19) the Hebrew has *Let the Lord smell* (or *inhale*) *an offering*. In Lev. 26, 31 JHVH says: *I will not smell the savor of your sweet odors.* When Noah after the Flood offered burnt-offerings, the Lord *smelled the sweet savor*, and the cuneiform account of the Deluge states that when the Babylonian Noah offered a sacrifice, the gods gathered around him like a swarm of flies, so that the goddess Istar took the great fly-brushes of her father Anu, the god of heaven, to drive them away. Fly-brushes are the ancient Oriental symbols of sovereignty. The gods were starved because there had been no offerings during the Flood (JAOS 41, 181).

The Hebrew term for the *fragrant smoke* of the burnt-offering is *qĕṭôrt*, and *nectar* seems to be derived from the same Semitic stem, just as it has been suggested that *ambrosia* may represent the Semitic *'ambar*, ambergris (EB[11] 1, 800[b]; AJSL 23, 261; PAPS 46, 158). Ambergris is a morbid secretion of the intestines of the sperm-whale. It is a fatty, inflammable substance which develops a peculiar sweet odor on exposure to the air. It plays an important part in Oriental perfumery and is used also in pharmacy and in cookery. I have shown in my paper on *Jonah's Whale*, which I presented at our General Meeting in 1907, that there were sperm-whales in the Mediterranean (PAPS 46, 155; JHUC 296, 37-43). Gr. *thýos* and *thýoma* are equivalent to Heb. *qĕṭôrt*, and both are connected with our *fume*, as is also *thysía*, and *tethyoménos* means *fragrant*. Similarly Heb. *mĕquṭṭâr* signifies *perfumed* in Cant. 3, 6. AV uses *perfume* for *qĕṭôrt* (JBL 36, 91, n. 11) in Exod. 30, 35.

[a] See the translation of *Leviticus*, in the Polychrome Bible, p. 62, l. 2; p. 63, l. 15.

The Arabic equivalent of the stem of Heb. *qĕṭôrt* means *to exhale an odor in roasting*. If you return to camp in the evening after having been out gunning all day, the smell of frying bacon is a *sweet savor*. The Hebrews sacrificed to JHVH the fat of the victim. Lev. 3, 16 states: *All the fat is the Lord's* (cf. Lev. 7, 25; 1 S 2, 16; 2 Chr. 7, 7; Gen. 4, 4). The fat pieces burnt on the altar were, according to Lev. 3, 3.4, the fat that covers the entrails, *i.e.* the great omentum, and the fat that is about the entrails, *i.e.* the mesenterial fat, the two kidneys and the fat that is on them, and the caudate lobe of the liver. The priests said the fat was the best and richest part of the animal.[7] Liver and kidneys and the surrounding fat were regarded as important seats of life and emotion. We find in the Hebrew psalms: *my liver exulted* for *I was glad* and *my reins admonished me at night* for *Thou art never out of my thoughts* (JAOS 32, 124; JHUC 325, 39).

The practice of offering the fat pieces appears in a new light if we compare a story of Prometheus in Hesiod's *Theogony* (*c.* 735 B.C.) in which the Bœotian poet describes the origin of the world and the birth of the gods. Socrates, who drank the cup of hemlock in 399, regarded the stories of the gods as the inventions of lying poets (EB[11] 25, 333[a]). According to Hesiod (*Theog.* 536), gods and men met on a certain occasion at Mecone, which is the ancient name of Sicyon, near the Gulf of Corinth, 10 m. N.W. of Corinth. The business of the assembly was to decide what portions of the slain animals the gods should receive in sacrifice. On one side Prometheus arranged the best parts of the ox, covered with offal; on the other, the bones covered with fat, as the meat was covered in Homeric sacrifices. Zeus was invited to make his choice, chose the fat, and found only bones beneath. Similar fables recur in Africa and North America (EB[11] 22, 436[a]).

If *nectar*, which is connected with Heb. *qĕṭôrt*, fragrant steam of the burnt-offering inhaled by JHVH, appears in the Homeric poems as the *drink* of the gods, we must remember that the Arabic term for *to smoke tobacco* is *to drink smoke*, Arab. *šáriba-'d-duxâna*. The same term was formerly used in English. Ben Jonson (1598) says:

[7] See the translation of *Leviticus*, in the Polychrome Bible, p. 65, ll. 33-38.

The most divine tobacco that I ever drunk. In the Oriental tobacco-pipe known as *narghîleh* (Arab. *nârgîlah* or *arkîlah*) the smoke passes through water before it is inhaled through a long flexible tube. The receptacle for the water, which is often scented, was originally a cocoanut shell. In Persian the cocoanut is called *nârgil*. In India a similar pipe is known as *hubble-bubble* (or *hobble-bobble*). Other names of this water-pipe for smoking are *hookah* (Arab. *ḥúqqah*) and *kalian* (Perṣ. *qaliān*).[8] In Egypt it is called *šîšah* or *gáuzah*. The Arabic name for *cocoanut* is *gôz Hindî*. The Arabic word for *to drink* appears in our *sherbet* and *syrup*. Some men in Waukegan, Ill., *smoke their hootch* now by taking a liberal pinch of snuff, soaking it in moonshine until it is thoroughly saturated, then cramming it into a pipe, and pressing a little loose tobacco on top (The Baltimore *News*, April 27, 1922, p. 16, col. 6).

In a poem *The Cigarette*, by Joseph Mills Hanson (published in *The American Legion Weekly*, April 28, 1922, p. 4, col. 2), we find the stanza:

> But how I longed to smoke—and not a snipe![9]
> When comes this long-legg'd bird that saved my tripe[10]
> Back in the boyau[11]—volunteer, may be,
> Or one of our supports—and handed me
> A Lucky![12] Boy, just listen while I state
> I'm here to tell the world this one thing straight,
> No Mount Olympus god could ever quaff
> A cup of nectar sweet as that, by half!

I am indebted for this reference to Dr. O. R. Sellers, of the Johns Hopkins University.

In Homer, *ambrosia* is used as a perfume: in the Odyssey (4, 445) we read that when Menelaus wanted to consult the old man of the sea, Proteus, who knew all things, past, present, and future, and who took siesta, surrounded by his seals, in an ocean-cave near the mouth of the Nile, the daughter of the god covered the hero and three of his companions with hides of seals, and in order to make the odor of the hides less intolerable, she put ambrosia under their noses. The ancients had no scents dissolved in alcohol, but perfumed greases,

[8] See cuts in CD 2878[a], 2908[a]; cf. EB[11] 13,670[b]; 19,240[a].

[9] Stub of a cigar or cigarette. [10] Cf. *to save one's bacon*.

[11] Passage between two trenches. [12] A *Lucky Strike* cigarette.

solid or liquid fats charged with odors. Pliny's statement (13, 2) that scented unguents were unknown at the time of the Trojan war is incorrect. Fats and oils absorb odors. Perfumes are extracted from flowers by the agency of inodorous fats (*enfleurage*). One of the most precious unguents was the nard-ointment, and according to Pliny (12, 43) nard-oil had a red color (*color rufus*). Also the color of myrrh, which was used as a perfume (Ps. 45, 8; Prov. 7, 17; Cant. 1, 13; 5, 5) and as an antiseptic for embalming (John 19, 39), varies from pale reddish-yellow to red or reddish-brown. Achilles's mother, Thetis (JHUC 306, 34) injected ambrosia and red nectar (Gr. *néktar erythrón*) through the nostrils of his slain friend Patroclus to preserve his body (*Il.* 19, 40). According to Herodotus (2, 86) the Egyptian embalmers removed the brains through the nostrils by means of a bent iron implement, injecting drugs, while the intestines were drawn out through an incision in the left side, whereupon the abdominal cavity was cleansed with date-brandy (JHUC 287, 33) and filled with myrrh, cassia, and other materials, and the opening sewed up; finally the body was steeped for 70 days in a solution of natron, *i.e.* native carbonate of sodium, which is found in some of the lakes of Egypt. On the other hand, the body of Alexander the Great is said to have been embalmed with honey (EB^{11} 9, 306[a]).

At the command of Zeus, Apollo bathed the body of the Lycian prince Sarpedon, who had been slain by Patroclus, in a river and anointed it with ambrosia (*Il.* 16, 670.680). Hera cleansed (Gr. *kátheren*) herself with ambrosia and anointed herself with fragrant ambrosian oil (*Il.* 14, 170; *cf.* Judith 16, 8). This was no soap, as has been suggested, but a scented massage cream. Massage, which is the oldest of all therapeutic means, is alluded to in Homer: in the Odyssey heroes returning from battle are rubbed and kneaded by female massers. *Massage* is derived from Arab. *mássada* (cf. Syr. *mĕšâšâ*, touching, groping; Heb. *mašáš*, to grope; Ass. *mašâšu* and *pašâšu*, to rub; JBL 39, 159). In Est. 2, 12 massage is called *tamrûqîm*, rubs: the oil of myrrh had an antiseptic effect and purified the skin; the balms or sweet odors perfumed the body; the rubs made the skin white and soft, and improved the figure (*Est.* 22). *Shampoo*

is the Hindoo term for this manipulation; Hindustani *châmpo* is the imperative of *châmpnâ,* to thrust, to press (EB[11] 17, 863[b]). Some of our modern massage creams are said to cleanse all dust and dirt from the pores; after they have been rubbed in gently they roll out, bringing with them all the dirt and skin impurities, so that the skin appears clean and healthy with a clear and glowing color, while the cream that comes from the pores appears darkened and dirt-laden (SEP, April 15, 1922, p. 93). According to Pliny (28, 191) soap was an invention of the Gauls, who prepared it from tallow and ashes. The ancients cleansed themselves by oiling their bodies and scraping (Gr. *stlengízein*) their skins, and by baths (EB 4665). Cowper (1791) says: *Her lovely face | She with ambrosia purified.*

Ambrosia is supposed to be connected with Skt. *amrta,* which denotes the beverage of immortality that resulted from the churning of the ocean by the gods and demons (CD s. *amrita*). The view that Gr. *ambrósios* means *immortal* is untenable. Nor can Gr. *néktar* be combined with Gr. *nógala,* dainties. The ancients regarded *néktar* as a compound of the negative *ne* and *kér,* the goddess of death, or *kteínein,* to kill. Our post-Volsteadian nectars may not always kill, but they certainly do not impart immortality.[13] Homer applies the epithet *ambrosial,* not only to divine food and anointing oil, but also to raiment, sandals, locks. A sexagenarian knows that hair is not immortal, and if he raised a number of boys he will remember that shoes have no everlasting soles. Ambrosial curls denotes *fragrant hair.*[14] Milton says (*Par. L.* 5, 57): *His dewy locks distilled ambrosia.* In Swift (*Streph. and Chloe*) we find: *Venus like her fragrant skin | Exhaled ambrosia from within.* The Scottish poet, Sir William Mure (1594–1657) has (*Dido and Æneas* 1, 461): *Her sweet ambrosial breath and nectared hair.* Our poets also speak of *nectarine kisses* or *a touch of her sweet nectar-breathing mouth.*[15]

[13] Littré says *s.* nectar: *Ce qui ne tue pas* does not signify *ce qui donne l'immortalité.*

[14] *Cf.* Fr. *chevelure ambroisienne,* Ger. *ambrosisches Haar.* We find also *ambrosische Nacht.*

[15] German poets speak of *Nektarlippen* and *Nektarküsse.* Schiller says: *Nektarduft von Mädchenlippen;* Wieland: *der Anhauch ihres Nektarmundes;* Rückert calls the lips *Nektarkelch.* We also find *nektarne Brust* (cf. *BL* 70, 72). Tennyson (*The Miller's Daughter*) says: *I would be the necklace— . . . upon her balmy bosom.*

The night is often called *ambrosial;* this does not mean *holy,* as is generally supposed, but *balmy.* Thomas Moore (*Lalla Rookh* 248) speaks of *One of those ambrosial eves | A day of storm so often leaves.* Tennyson (*In Memoriam,* lxxxvi) says: *Sweet after showers, ambrosial air* and (*Œnone*): *A fruit of pure Hesperian gold | That smelled ambrosially* (see OD s. *ambrosial*). Also *ambrosial sleep* means *balmy sleep,* i.e. *healing,* refreshing sleep. Edith M. Hull says in the first chapter of *The Sheik:* It was a wonderful night, silent except for the cicada's monotonous chirping, mysterious with the inexplicable mystery that hangs always in the Oriental night. The smells of the East rose up all around her; here, as at home, they seemed more perceptible by night than by day. Often at home she had stood on the little stone balcony outside her room, *drinking in the smells of the night*—the pungent, earthy smell after rain, the aromatic smell of pine trees near the house. It was the *intoxicating smells of the night* that had first driven her, as a very small child, to clamber down from her balcony, clinging to the thick ivy roots, to wander with the delightful sense of wrongdoing through the moonlit park and even into the adjoining gloomy woods. She had always been utterly fearless.

There is no connection between the Gr. *nectar and ambrosia* and the Biblical *manna.* The manna, which sustained the ancestors of the Jews in the wilderness, was a nutritive lichen like the Iceland moss and the reindeer moss, especially the *Lecanora esculenta,* known as *manna-lichen,*[16] which in times of great drought and famine has served as food for a large number of men in the arid steppes of the various countries stretching from Algeria to Tatary (EB[11] 16, 584). Fragments of manna-lichen carried away by the wind resemble grains of wheat. They vary in size from a pea to a hazel-nut.[17] The edible lichens contain not only starchy substances, but also in some cases a small quantity of saccharine matter of the nature of mannite. It is

[16] Littré says *s.* manna: *Il est certain qu'elle est formée de lichens, surtout de lecanora affinis et lecanora esculenta.*

[17] According to Num. 11, 7 the manna was like coriander seed. The smooth globular fruits of *coriander sativum* are twice as large as hemp seed or about the size of a peppercorn. The Hebrew word in Exod. 16, 14, rendered *round* in AV means *flaky;* see RV, margin; *cf.* EB 879, n. 4.

more probable, however, that the powdered manna-lichen was mixed with tamarisk-manna and alhagi-manna (Arab. *taranjabîr*). The manna-lichen was ground in querns or pounded in mortars (Num. 11, 8) and mixed with the honey-like drops exuding from the soft twigs of the *tamarix Gallica* or with the exudation of the camel's thorn (*alhagi Maurorum* or *camelorum*). After this mixture of powdered manna-lichen and tamarisk-manna or alhagi-manna had been baked in baking-pots, it tasted like honey-cake (Exod. 16, 31) or like pastry baked in sweet-oil (Num. 11, 8).

The real meaning of the name *manna* has never been explained. Arab. *mann* means not only *manna*, but also *gift*, present, favor, benefit; it denotes also the *manna-insect* which causes the secretion of the manna by puncturing the twigs of the tamarisk (*i.e.* the *Coccus manniparus* or *Gossyparia mannifera*). The presence of these insects may be responsible for the legend that when some of the manna was left until the following day, it became wormy and offensive except on the sabbath (Exod. 16, 20.24). The accounts given in Exod. 16, 14–36; Num. 11, 7–9 are inaccurate and embroidered. The primary connotation of Heb. *man*, manna, is not *gift*, but *separation*, elimination, secretion. It is connected with the preposition *min*, from, which means originally *part* (VS 397; GB[16] 435[a], 4; GK[28], § 119, w, note 1). *To part* may mean *to partition*, apportion. Arab. *manîiah*, fate, signifies properly *portion* (Heb. *mĕnât*, *ḥelq*). This is also the primary connotation of Arab. *mann* and *minḥah*, gift, present. AV uses *to part* for Heb. *hifrîd* in Ruth 1, 17, where Ruth says to Naomi: The Lord do so unto me and more also if aught but death part thee and me. Here Luther has: *Der Tod muss mich und dich scheiden*, and *Ausscheidung* is the German term for *secretion*. Arab. *mâna*, *iaminu*, to plow, is *to break* the ground. The original meaning of Heb. *mîn*, species, is *division*. Lat. *species* means not only *particular sort*, but also *look*, form (Heb. *tĕmûnâ*; cf. BA 1, 124). The post-Biblical *mîn*, heretic, signifies properly *separatist*. Brugsch and Ebers combined Heb. *man* with the late Egyptian *mnu*; if this denote *manna*, it is no doubt a loan-word, so that it throws no light on the etymology.

In Exod. 16, 15 the name *manna* is derived from *mân-hû*: when the ancestors of the Jews saw it, they said to one another: *mân-hû*,

what is this? for they did not know what it was. *Mân-hû,* however, is Aramaic, not Hebrew. The Syriac Bible has *mânâu* = *mânâ-hû* in Exod. 16, 15. In Syriac we find *mân* or *môn*, and *mânâ*, what, but the Hebrew pronoun for *what?* is *mâ*. The popular etymology given in Exod. 16, 15 must be a late gloss. AV has *What is this?* in the margin, also *It is a portion.* In the text AV renders: *It is manna.* RV has in the text *What is this?* and *It is manna* in the margin.

Tamarisk-manna is alluded to by Herodotus (7, 31). He says in his account of Xerxes's march to Sardes during his expedition against Greece (about 481) that the Callatebian craftsmen prepared honey from tamarisks and from wheat (Gr. *ándres demioergoì méli ek myríkes te kaì pyroû poieûsi*). In the OT the term *honey* denotes also various inspissated *fruit-juices* or syrups, especially *grape-syrup* (Gr. *hépsema, síraion,* Arab. *dibs*). Callatebus was a town in Lydia S.W. of Sardes, probably near the Lydian Philadelphia, the present Alashehr, 83 m. E. of Smyrna. This Philadelphia was called Little Athens on account of its festivals and temples. It was captured in 1402 by Timur (or Tamerlane) who built a wall of the corpses of his prisoners. The tamarisk-honey is tamarisk-manna, and the honey prepared from wheat may have been glucose made from wheaten starch (Plin. 18, 76) by the action of dilute sulphuric acid. This acid, which is perhaps the most important of all chemicals, was, it may be supposed, known to the ancients (*cf.* Plin. 35, 175), while hydrochloric acid was first obtained about the end of the Thirty Years' War (1648). Sulphuric acid is found uncombined in natural waters of certain volcanic districts. The Lydian Philadelphia was subject to frequent earthquakes. The Mæander valley and the Gulf of Smyrna are notorious seismic foci (EB^{11} 2, 757^b). The Mæander valley is noted for its hot springs. The Lydians were credited with several inventions, *e.g.* dice and coined money. They were also celebrated for their music and gymnastic exercises. The Lydian empire was the industrial power of the ancient world.

APPLICATION OF BIOPHYSICAL RESEARCH TO MEDICAL PROBLEMS.

By G. W. CRILE, M.D. and HUGO FRICKE, Ph.D.

The research, part of which is reported in this paper, is a further extension of previous studies undertaken for the purpose of ascertaining to what extent biophysical methods can be used in the investigation and interpretation of medical problems. Our observations of the effect of certain agencies such as adrenalin, anesthetics, stimulants, electrolytes, etc., on the temperature of various organs and tissues of the body have been made with the thought that if the effect of these influences should prove to be uniform and consistent with biological facts, these studies would lead to a wider use of biophysical methods. There are many other studies which will be reported in a later paper in which we expect to set forth an interpretation of our findings. At the present time we wish only to make known the facts which have appeared in these investigations.

Several similar attempts to study temperature changes in the tissues of animals under different conditions have been carried out in the past. In addition to the literature reported in a previous publication, we may especially mention the work of Mosso,[1] who measured the temperature changes by means of a very sensitive mercury thermometer. He believed that he had found a very great change in the metabolism of the brain followed the injections of certain drugs, among them being absinthe and strychnin. Hill and Nabarro,[2] however, criticized Mosso's work and made evident that his results were due mainly to the action of the drugs upon the blood circulation.

The amount of oxygen used in the metabolic processes in a state of rest and of excitation is known for many of the body organs in which excitation usually causes a several-fold increased consumption. How the energy corresponding to this oxygen consumption is used; how much is transformed into heat; and how much is used by the organ in the performance of its functions, are questions whose an-

[1] Mosso, *Proc. Roy. Soc.*, London, 1892.

[2] Hill, L. and Nabarro, D. N., *Jour. Physiol.*, 1895, XVIII., 218.

swers have not been directly determined for most organs. In the case of a muscle at work, A. V. Hill[3] has found that there is a maximum efficiency of fifty per cent. From an estimation of the work done by the organs it is safe to assume that for most organs the major portion of the chemical energy obtained through the oxygen consumption is directly transformed into heat. The chemical energy of the oxygen consumed by an organ under excitation corresponds to a temperature increase of a few tenths of a degree Centigrade per minute. The magnitude of this change indicates the feasibility of employing thermocouples for the temperature measurements in the study of metabolic processes.

In the studies reported here our attention has been directed especially to the measurement of the temperature changes which occur in the brain. As compared with most other organs, the oxygen consumption of the brain is large. It seems probable that the energy corresponding to this consumption is not directly converted into heat for the sole purpose of maintaining the brain temperature, but that it is primarily used by the brain in its special activities, finally appearing as heat in the brain or in other parts of the body. If this is so we would expect that activation of the brain would be accompanied by temperature changes large enough to be recorded.

It is evident, however, that changes in the circulation of the blood, due to vaso-constriction and vaso-dilation in the different parts of the body, must be an important factor in the production of the temperature changes in an organ. This fact makes the interpretation of the records of temperature change quite complicated.

Experimental Technique.—The temperature changes in the organs studied were measured by means of copper-constantan thermocouples combined with mirror galvanometers. The time of vibration of the galvanometers was about seven seconds. The galvanometer deflections were reflected on a common scale, the resistances in each circuit being so adjusted that a deflection of one division on the scale (equal to two millimeters) corresponded to a temperature change of one one-hundredth of a degree Centigrade. The scale covered a temperature range of 6° C. A specially designed potentiometer made possible the immediate introduction into each thermo-

[3] Hill, A. V., *Jour. Physiol.*, 1913, XLVI., 435.

couple circuit of an electromotive force corresponding to a temperature difference of 4° C., so that continuous temperature readings could be made over a range of 14° C.

One junction of each thermocouple was inserted in a constant temperature bath, constant to within one one-hundredth of a degree Centigrade, the other junction being inserted in the organ under investigation. In most measurements one thermocouple was inserted in the brain and one in some other organ or tissue. In some cases, however, three thermocouples were employed.

In order to avoid the influence of bioelectric, or galvanic forces, it is necessary to insulate thoroughly the wires that are inserted in an organ. This was done by enameling the wires and coating them with de Khotinsky cement.

A special metal holder was constructed for the thermocouple that was inserted in the brain. The wires, constantan No. 35 and copper, No. 40, were passed through a hard rubber cylinder, the thermojunction extending about one centimeter beyond the end of the rubber, the exposed portion being covered with de Khotinsky cement which served also to cement the thermojunction in place.

The skull of the rabbit was trephined to make an opening 6 mm. in diameter and the holder clamped in place, the hard rubber rod with the thermocouple junction being then inserted. The depth of penetration of the brain varied in different experiments from 2 mm. to about 8 mm. The trephined opening was made to the right of the mid-line on a level with the posterior superciliary ridge.

The thermocouples inserted in other organs also were constructed of No. 35 constantan and No. 40 copper wire. The wires were soldered together, the constantan wire extending about three inches beyond the junction. By passing this free end through a needle the junction could be readily drawn into the desired position within the tissue.

We wish to express our appreciation of the coöperation of Mr. Seitz of the Electromechanical Engineering Department of the Cleveland Clinic in the construction of the apparatus described above.

The accompanying group of curves shows the effects of various drugs upon the temperature of certain tissues of rabbits, especially of the brain. In a later paper we propose to discuss the relative

rôles played in these temperature changes by alterations in the blood circulation, in the blood temperature, and in the metabolism of the organ.

Inhalation anesthesia.—*Ether* in its first stages frequently produced an increase in the temperature of the brain. As the depth of the anesthesia was increased the temperature fell. Fig. 1 shows the

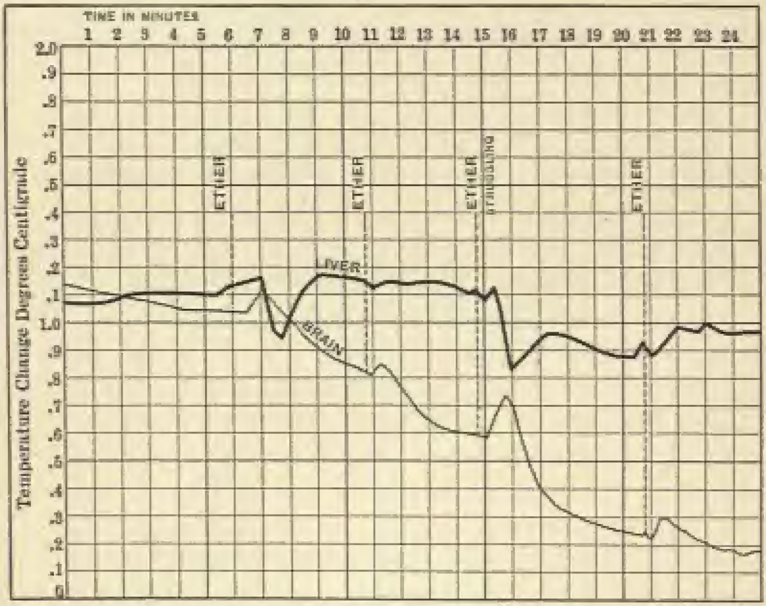


FIG. 1. Effect of the administration of small amounts of ether on the temperature of the brain and of the liver.

effect of the successive administration of small amounts of ether, the intervals being long enough to allow the animal to become nearly conscious between the doses.

Nitrous-oxide as compared with ether showed a less marked change in the temperature and fewer fluctuations, thus giving a more stabilized curve (Fig. 2).

Chloroform showed an initial rise in temperature corresponding to that produced by ether. When full anesthesia was established the temperature fell.

Adrenalin: A number of observations have been made of the effect of the intravenous injection of adrenalin upon the temperature

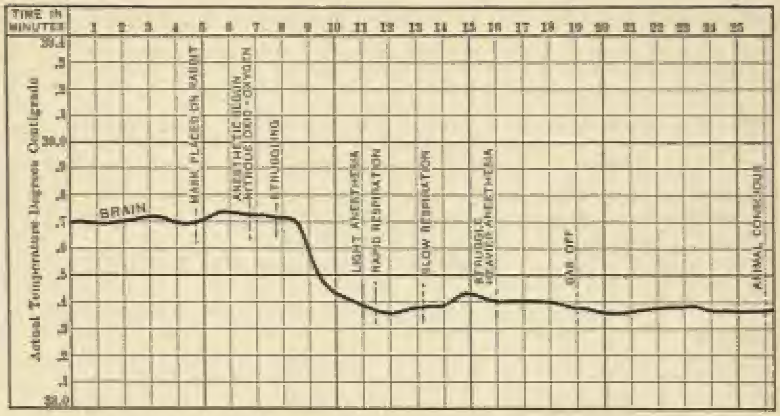


FIG. 2. Effect of prolonged nitrous oxid anesthesia on the temperature of the brain.

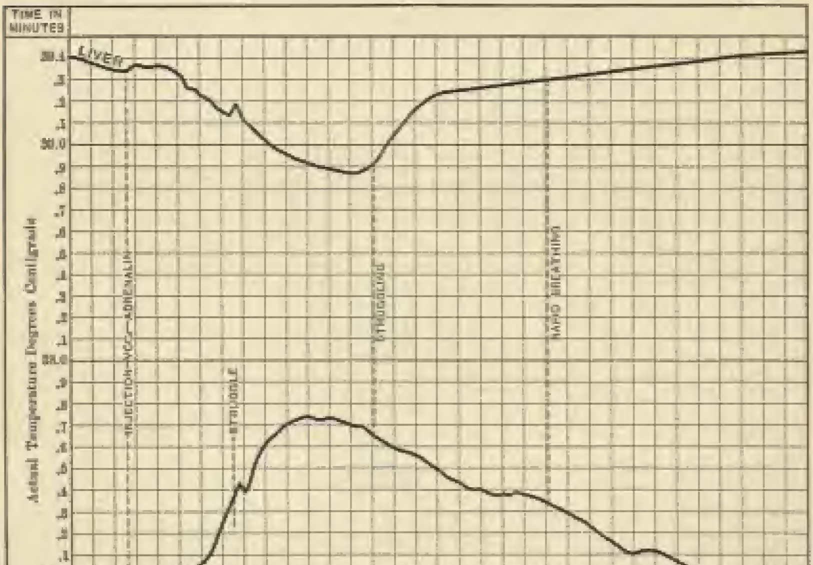


FIG. 3. Effect of the injection of adrenalin upon the temperature of the liver and of the brain.

of the brain, the liver, the thyroid gland, the adrenal glands, the spleen, the intestines, the kidneys, and the muscles (Figs. 3–5).

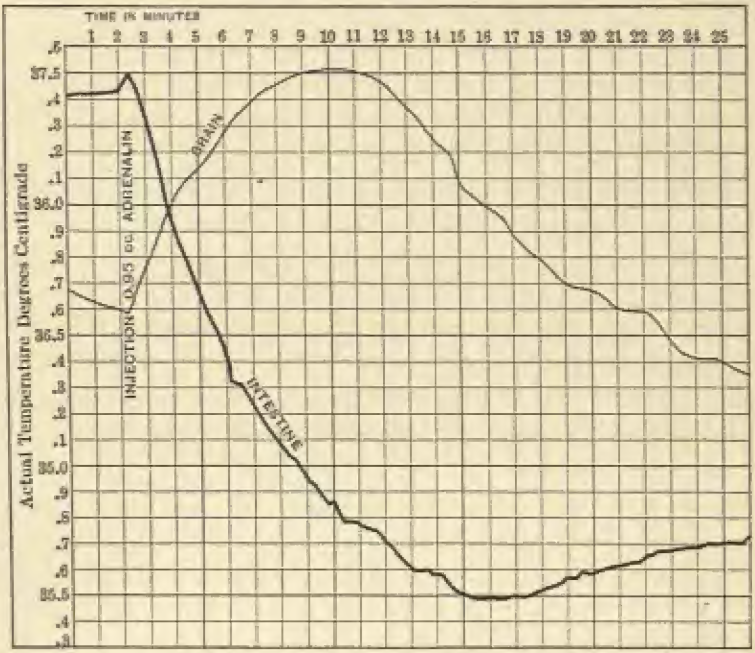


FIG. 4. Effect of the injection of adrenalin upon the temperature of the intestine and of the brain.

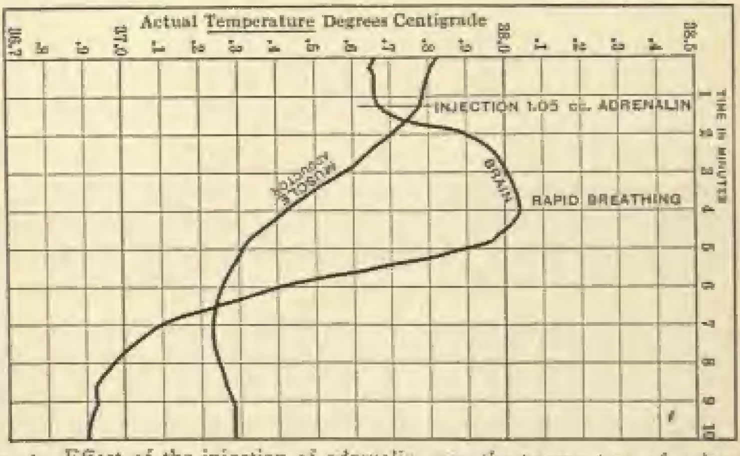


FIG. 5. Effect of the injection of adrenalin upon the temperature of voluntary muscle and of the brain.

The uniform dosage employed throughout was the same as that used in previous researches, viz., 0.4 c.c. per kg. of 1 : 1000 adrenalin chloride (P. D. & Co.).

In every case, the brain showed a very characteristic rise in temperature, the temperature increase continuing for from ten to fifteen minutes and amounting, on an average, to about 0.7° C. The other organs usually show an effect just opposite to that in the brain, the minimum temperature, however, usually occurring a few minutes later than the corresponding maximum for the brain.

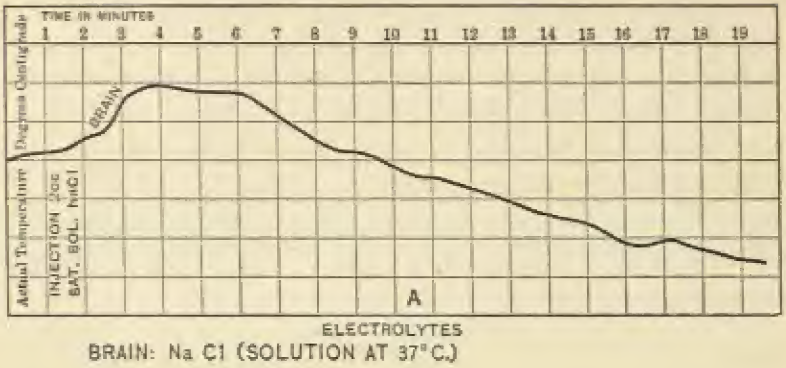
In several cases the temperature of the spleen and of the intestine fell markedly, in some cases as much as two degrees or more (Fig. 4).

In all the above experiments the thermocouple was inserted in the gray matter of the brain and in every case a rise in temperature was noted. It was found, however, that when the thermocouple was placed in the white matter, no change in temperature occurred after the injection of adrenalin.

Amyl Nitrite: The inhalation of amyl nitrite produced a marked rise of 1.1° C. in the brain temperature, the liver showing no decided effect, excepting a slight, continuous decrease in temperature.

Electrolytes: The action of sodium and calcium chlorides has an important theoretical interest. Fig. 6 shows the effects of the injection of these salts upon the temperature of the brain. The well-known antagonistic relation existing between sodium chloride and calcium chloride is strikingly illustrated. The doses given were 2 c.c. of a saturated solution—36 per cent.—of sodium chloride and 1 c.c. of a 10 per cent. solution of calcium chloride.

Sodium Cyanide: The action of a strong poison was shown by various injections of sodium cyanide. The injection of 0.001 *N* solution caused a slight rise of temperature, corresponding to the stage of excitement. The injection of 0.01 *N* solution produced a state of depression, while an injection of 0.1 *N* solution caused an immediate drop in temperature followed by a marked rise; during a period in which violent tremors and convulsions were occurring. During these convulsions the temperature fluctuated over a range of 0.1° C. The result of the injection of still stronger doses was an immediate, rapid decrease in the temperature of the brain followed in a few minutes by death.



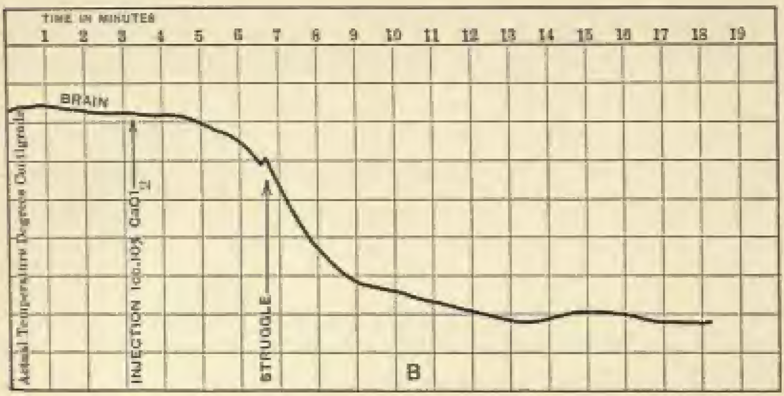


FIG. 6. Effect upon the temperature of the brain of the intravenous injection of (*A*) 2 c.c. of NaCl (saturated—37° C.); (*B*) 1 c.c. of $CaCl_2$ (10 per cent. solution).

SUMMARY.

1. By means of direct measurements of the variations in the temperature of animal tissues, new light may be thrown upon the action of any agent upon the organism.

2. The observations thus far made are consistent with the clinical observations, and with the findings in other forms of physiological research.

3. The opposite effects of the injection of adrenalin upon the brain and upon other organs and tissues demand especial consideration.

4. The use of the biophysical method of measuring variations in function opens the way to further applications of biophysical methods in the study of physiological problems.

THE AGE OF THE EARTH FROM THE GEOLOGICAL VIEWPOINT.

By T. C. CHAMBERLIN.

(*Read April 22, 1922.*)

General Introduction.

In pioneer days, when the sciences were struggling for a place in the sun, it fell to Geology to pull up and set back the stakes that man had stuck to mark the beginning of the earth. This seemed to many a moving of sacred landmarks; to others it seemed a wanton use of the secrets of the cemetery of nature's dead. A bitter war arose: racial bias disputing the rock beds; tradition and sentiment fighting mud layers and fossil imprints. The struggle that followed was long. The throwing of rocks and rock-ribbed arguments grew to be an art that might well have drawn forth the envy of an Ajax. But the substantial slowly gained on the sentimental. The brutal cogency of a slab of fossils could be hated and fought, but could not be gainsaid. And as the tide turned, the geologist began to play crusader; he mounted his war horse and went forth to convert the world—including, withal, some of his scientific colleagues.

After a time, however, the battle shifted to another field. Darwin and Wallace drew off a following and taught them to use the subtler weapons of "the struggle for existence" and "natural selection." However, they still plied the old geologic weapons, for they, too, had reason to point to bed on bed; they had need of even more time than the geologists. So they took the lead and the team became a tandem, Biology prancing in front, Geology trotting on in the thills.

But the spirit and abandon of this team soon awakened a new antagonist. Kelvin took the field in the name of Physics, Astronomy and Mathematics, and sought to set metes and bounds to the backward extension of terrestrial time. He told the tandem, with much show of premises and figures, that the feed on hand positively would

not let them go as far as they proposed. The tandem was reined in and marked time, losing not a little of the free natural pace it should have retained.

But in time this great antagonist was neatly flanked from an unexpected quarter. Certain physicists and chemists discovered that they had a decaying atom on hand. They keenly watched its rate of decay and soon came to see that if atoms take as long to grow as some of them take to die off, there should have been time enough for this little ball of atoms to get together—and plenty of energy as well.

So, too, astronomers began to see that the making of globular clusters and stellar galaxies required time. If 60,000 suns have time to come together and work themselves into a steady state while yet they are suns, the getting together of our little earth may be merely a negligible matter after all. And so a new order of things has arisen. The tandem is a vexed tandem no longer. We now have a fine four-horse team; Astronomy and Physics at the front, leading off at a great pace; Biology on the pole, steadying the team, and Geology plodding on as the old original wheel horse.

The Geologic Problem.

Now, I must hasten to warn you not to expect much of the old wheel horse. He has grown stiff in his paces, and his paces are not what they should have been. Kelvin checked him too high. A reasonable check should have given him good form and some sense of restraint, but checked too high, he took to short mincing steps. As a result he's in poor shape to swing into the great pace of the new leaders. It is too much to expect him to recover his natural step at once, but he will in time. For the present, he will need a touch of the whip now and then to make him keep pace. Let this be gentle and considerate, because of his age and his past service, but let it be persuasive.

Representative Geologic Time-estimates.

It is a simple matter, theoretically, to use the rate at which sediments are being laid down, or solutions gathered into the ocean, as a divisor to find the time required to lay down the whole column of sediments or the whole accumulation of the salts in the sea. Practi-

cally there are serious difficulties. In the first dozen years of this century four notable estimates were made in this way by able geologists: two American, Clarke[1] and Becker;[2] two British, Joly[3] and Sollas.[4] These estimates form an admirable point of departure for this discussion. They represent the mode of geologic interpretation that has been most current until recently; they typify opinions widely held by the conservative school of geologists; they stand out in contrast to the views of the new school.[5] The mean of the four estimates, on the basis of the sediments, was 90,000,000 years, roundly; on the basis of the ocean, 95,000,000 years. The highest individual estimate was 150,000,000 years; the lowest 70,000,000 years. I shall not deal with the individual estimates, but merely with their mean value, and with that only as representative.

My discussion can not be specific and concrete without some reference to views in other fields. My colleagues in this Symposium will give you the last word from their viewpoints, and if I could follow them I would gladly take their estimates as specifically representative in their several lines. In lieu of this, I can only use such general views as are current. It has long been known to be the view of many biologists that the evolution of life required much more than 100,000,000 years. It is also well known that most estimates based on

[1] F. W. Clarke, "A Preliminary Study of Chemical Denudation," *Smiths. Misc. Coll.*, LVI., No. 5 (1910); "The Data of Geochemistry."

[2] George F. Becker, "The Age of the Earth," *Smiths. Misc. Coll.*, LVI., No. 6 (1910).

[3] J. Joly, "An Estimate of the Geological Age of the Earth," *Trans. Roy. Soc. Dublin*, VII. (1899), pp. 23–66; "The Age of the Earth," *Phil. Mag.*, 6th ser., Vol. XXII. (1911), pp. 359–80.

[4] W. J. Sollas, "Presidential Address," *Quart. Jour. Geol. Soc.*, Vol. 65 (May, 1909), *Proc. Geol. Soc. of London.*, Sess. 1908–9, pp. l–cxxii.

[5] It is not practicable to summarize the time-estimates of the newer school consistently with the division of labor adopted in this Symposium since they are composite, embracing organic, astronomic, and radioactive factors, with some emphasis on the last. The following papers of this class may be taken as representative: J. Joly, "Radioactivity and Geology," Van Nostrand, New York (1909), pp. 233–51; Arthur Holmes, "The Age of the Earth," Harper Bros., London and New York (1913), pp. 1–196; J. Barrell, "Rhythm and the Measurement of Geological Time," *Bull. Geol. Soc. Am.*, Vol. 28 (1917), pp. 745–904; T. C. Chamberlin, "Diastrophism and the Formative Processes," XIII., "The Time over which the Ingathering of Planetesimals was Spread," *Jour. Geol.*, Vol. XXVIII. (1920), pp. 675–81.

radio-activity greatly exceed this. Astronomical opinion has recently been trending toward the view that long periods are necessary for certain typical phases of celestial evolution. Perhaps I may overstep my proper limits far enough to say that I have recently tried to form some notion of the time required for the gathering of planetesimals from what seemed a probable distribution into the collecting planetary nuclei, and found a period of the order of two or three billion years the most probable.[6] These current views in the collateral fields warrant me in assuming that there is a wide discrepancy between the geological estimates just cited and the present estimates in the related fields. In view of this I can perhaps serve you best by inquiring whether the recent additions to geologic evidence and the newer modes of interpretation mitigate this discrepancy in any appreciable measure. Let us consider first what the newer evidence relative to the sediments has to say, and turn later to the solutions.

The Testimony of the Sediments.

In considering possible modifications of the foregoing estimates five questions arise: (1) How far do recent investigations tend to lengthen or to shorten the older estimates? (2) To what extent has human action made the present rate of wash and deposit faster than the mean pre-human rate? (3) How far does the present state of elevation make the present rate faster or slower than the mean rate of the past? (4) How does the present area of erosion compare with the mean area? And, finally, (5) does the lower end of the geologic column give us the point from which the accumulation of the sediments began? I can try to answer these questions only very briefly and inadequately.

1. *The Effect of Intensive Studies on Earlier Time Estimates.*— A strictly accurate chronology reaching back from the present for several thousands of years is now being worked out by De Geer.[7]

[6] Diatrophism and the Formative Processes, XIII., The Time over which the Ingathering of the Planetesimals was Spread, T. C. Chamberlin, *Jour. Geol.*, Vol. XXVIII. (1920), pp. 675–81.

[7] Gerald De Geer, "A Geochronology of the Last 12,000 Years," *Proc. Int. Congr.*, Stockholm, 1910, p. 241; "Kontinentale Niveauveränderungen im Norden Europas," ibid., p. 849; Spitzbergen, ibid., p. 1205; "Phenomenes Quaternaires de Stockholm," ibid., p. 1290; "Quaternary Phenomena in the Southern Part of Sweden," ibid., p. 1339.

He has succeeded in identifying the yearly deposits of glacial waters and in correlating them with annual moraines. In addition to this, he has been able to match sections at distant points by comparing the succession of peculiarities in the annual "varve" layers. While this is a quite special method and has only limited application, it is important to general time estimates, because it gives a means of checking up other criteria that indicate glacial time, and these help check up certain non-glacial criteria. As is well known, the duration of the recent Ice Age was for a time a sharply battled question, and the old views pitted against the new views came to be well defined. Though not yet ready for precise announcement, it is already foreshadowed that the De Geer method of measurement, when it shall have fully covered the retreatal stage of the last glacial epoch, will show that stage to be about three times as long as it was made by the most representative of the old estimates. The main differences of opinion as to the duration of the glacial period, however, grew out of the evidence that instead of one simple short epoch there were several epochs of glaciation separated by rather long interglacial intervals. Now, by using the De Geer method to correct the criteria on which time estimates of these glacial and interglacial epochs have been based, a glacial period at least twenty times as long as that assigned by the old estimate seems to be foreshadowed. Very likely this degree of extension of an old-time estimate by a new one is exceptional; at any rate, the glacial formations are exceptional deposits and make up only a small part of the geologic column.

In considering the standard water-lain sediments of the column, it is to be understood that only rapid surveys or mere reconnaissances have as yet been made of the larger part of the earth, and that inevitably inconspicuous breaks in the continuity of the deposits have been overlooked. As a result recent critical studies have revealed in some cases surprising numbers of gaps in the continuity of deposition. For example, Dr. Stuart Weller, in a study of what was formerly regarded as a continuous section of the Mississippian, has found no less than 12 breaks in continuity. The time-value of these, in his judgment, is two or three times as great as that of the visible beds themselves.[8] The time-values of such intervals are best judged by

[8] Personal communication.

comparing the faunas below them with those above, but this falls within the province of my paleontological colleague, and I therefore leave this source of correction in his hands, merely expressing the conviction that these breaks in the continuity of the sediments are quite sure, when finally and fully adjudicated, to extend greatly the old estimates of the time occupied in sedimentation.

2. *Human Acceleration of the Rate of Deposition.*—To pioneers who watched the effects of floods, freshets and ordinary wash on the native surface of our prairies and forests in their virgin state, and who are able now to compare this with the present wash of the same surfaces under cultivation, there is no need to argue that human intervention has greatly hastened denudation and deposition. In the native state the surface was protected by thick mats of grass, leaves and other vegetal debris; while the soil was bound together by dense entanglements of roots. The waters then ran almost clear where now they run mud.[9] Added to this are the quickened deflation of winds, the wear of roadways, the effects of quarrying and other excavation, as well as the actual carting away of clays, sands, gravels, quarry stone, foodstuffs, timber, and other material. While it is not easy to fix on a definite measure of these effects, the needed correction seems certainly to be large.

3. *Correction for the Present Elevation of the Surface.*—It is held by leading American geologists that the general elevatory movements of the continents have alternated with periods of relative stability during which the processes of base-leveling and sea-transgression have cut down the continents and developed peneplains.[10]

[9] A fuller statement of this with citations of data from Dole and Stabler and from F. W. Clarke is given in "Diatrophism and the Formative Processes, VIII., The Quantitative Element in Continental Growth," T. C. Chamberlin, *Jour. Geol.*, Vol. XXII. (1913), pp. 522–28.

[10] The following group of papers emphasize the rhythmical nature of elevation and stability and of the action of the atmosphere and hydrosphere upon the periodic deformations of the earth body and thus form the basis for the arguments in this and the next section: "A Group of Hypotheses Bearing on Climatic Changes," T. C. Chamberlin, *Jour. Geol.*, Vol. V. (1897), pp. 681–83; "The Ultimate Basis for Time Divisions and the Classification of Geologic History," ibid., Vol. VI. (1898), pp. 449–63; "A Systematic Source of Provincial Faunas," ibid., Vol. VI. (1898), pp. 597–609; "The Influence of Great Epochs of Limestone Formation upon the Constitution of the Atmos-

The periods occupied in the process of lowering the surface by denudation are held with good reason to be greater than those occupied in its elevation by deformative action. It is needless to say that elevation increases the velocity of the run-off, and that this velocity greatly increases the transporting power.[11] It is generally agreed that the present altitude of the continents is greater than their mean elevation during geologic history. Geologists recognize at least two stages in which the continents were exceptionally high and broad: that which attended the transition from the Paleozoic to the Mesozoic Era, and that which attended the transition of the Tertiary to the present epoch. The existing stage thus falls in one of the most notable stages when continental elevation and breadth were greatest, though perhaps not at its climax. Geikie estimates the present mean elevation of the land at 2,441 feet.[12] The mean elevation of the great peneplains is a matter of judgment rather than of knowledge, but no one would probably put the elevation at much more than a third of this. Probably a third is too high. The mean elevation for all the ages, high and low, quite surely falls somewhere between 2,400 and 800 feet, and probably nearer the 800 than the 2,400. There can be little doubt, then, that the present rate of denudation and deposition is much above the mean rate.

There are incidental conditions attending high relief which add appreciably to the immediate effects of the steep declivities to which it gives rise. Relief of the surface increases the vertical air currents and these favor precipitation; they also tend to concentrate the precipitation and give it enhanced effect. High relief often induces sharp showers and distinctly rapid run-off. The smooth surfaces of the stages of lower elevation, on the other hand, favor a more even

phere," ibid., Vol. VI. (1898), pp. 609–22; and specifically as applied to the question of age; "Rhythm and the Measurement of Time," J. Barrell, *Bull. Geol. Soc. Am.*, Vol. 28 (1917), pp. 745–904. The argument used in the present paper will be found stated as a quotation from Chamberlin in Holmes' "The Age of the Earth," cited above (1913), pp. 79–81. See also T. C. Chamberlin, "Diastrophism as the Ultimate Basis of Correlation," in "Outlines of of Geologic History," compiled by B. Willis and R. D. Salisbury (1910), The University of Chicago Press, pp. 298–306.

[11] See the special investigation of G. K. Gilbert, "The Transportation of Debris by Running Water," Prof. Paper 86, U. S. Geol. Surv. (1914).

[12] "Textbook of Geology," 4th ed. Vol. I. (1903), p. 49.

distribution of rain, a larger absorption into the soil, and a slower run-off of the remainder. So, too, accidented surfaces are likely to be ineffectively protected by vegetation, for lack of soil, or of adequate moisture. These and other incidental influences add appreciably to the total effect. It seems clear, therefore, that a large correction is to be made to the present rate of denudation because of the relatively high elevation of the continents.

4. *Correction for Area.*—This is to a large degree, but not wholly, an effect of the elevation of the continents, but none the less it deserves separate recognition. When elevation increases the land area, base-leveling and sea-transgression at once set in and combine their forces to reduce the exposed area. The result is very large variations in the areas of the ancient lands. The estimates of Schuchert and others for North America show variations that range from the full surface of the continental platform down to half that surface. As a rule, of course, the lesser surfaces were also low surfaces, and the two influences were cumulative. At stages of low elevation and slack drainage deep soils were likely to accumulate and these favored thick vegetation which helped to hold the soils. Thus in several ways small area and low elevation united their influence in a cumulative effect which could not have been other than large.

Partial Summary.—Summarizing at this point, it appears that four important corrections quite certainly must be applied to the present rate of geologic action to reduce it to a mean rate for the whole of geologic time. These corrections are cumulative. There seems to be no way at present to evaluate them rigorously or perhaps even very closely. The weighing of their value is greatly affected by the individual judgment, and that in turn by individual experiences and opportunities of observation. Speaking for myself alone, it does not seem to be overstraining the importance of these corrections to suppose that their cumulative value will be found great enough to bring the old-time estimates up to figures of about the same order as those of the current radio-active estimates.

5. *The Lower End of the Geologic Column.*—Below the base of the Paleozoic series the geologic terranes are much obscured by diastrophism and metamorphism. It remains to inquire what is the

testimony of this obscured portion as to the horizon at which the sediments began to be deposited, for that is essential to measuring the whole period of deposition. It was once thought that the Cambrian beds lay close upon "the original crust," and that they either represented the real beginning of the sedimentary series or else an early stage close to the beginning. But as field work progressed it was found that first one and then another thick series of sediments lay below the Cambrian. It was further found that there were marked unconformities between these great terranes, and that these were of such a nature as to imply long intervals of time unrepresented by deposits; that is, times when the deposition took place elsewhere. The number of such strongly unconformable terranes has been notably increasing as investigation proceeds. The correlation of these is not yet complete or even wholly satisfactory, so far as it has gone, but the leading workers in this field recognize six, eight, or more great stages. This Precambrian factor is thus certainly great, but just how great is yet undetermined.

The mere extension of the sediments downwards in this large degree is not, however, the most significant feature of this recent work. Great granitic series form a prominent feature of these lower terranes. These were formerly taken to be parts of "the original crust." They have been found, however, to consist of remarkable *intrusions* into earlier series made up of sediments, volcanic debris, and surface lava flows. The granites are not evidence of "the original crust of the molten globe." Nor does there seem to be any other trustworthy evidence of "an original crust." Thus observational evidence does not give the depth at which the bottom of the column of sediments is to be found, and theory is perhaps as favorable to a depth of a thousand or two thousand miles as any shallower depth. A reliable starting point for reckoning the total thickness of the sediments is not available.

THE TESTIMONY OF THE SOLUTIONS.[13]

In the effort to find the earth's age by means of sediments advantage may be taken of the fact that each deposit makes its own indi-

[13] Only a brief general statement could be made at the Symposium for lack of time. Adequate citation of evidence or of authority, or elaboration of

vidual contribution. It is thus possible to sum up as many of these separate contributions as can be measured satisfactorily and rest the case there, leaving what remains of the earth's age to be found out later or to be guessed at or to be ignored. But when the inquiry turns to the solutions it must face the fact that the contributions of each stage have been mingled with those of all other stages and the record to be measured is thus an indivisible unit. If the ocean, considered as such record, can be used to measure age at all, it is the total age of the ocean. This total age of the ocean can not be expected to tally with the age found from an unknown fraction of sediments.

The Basis of Estimating the Age of the Ocean.—The interpretation of the time occupied in the concentration of solutions in the ocean hangs on the assumptions made relative to its origin and to the entire history of the earth's waters on land and sea alike. This includes the volume of the waters at the start and all along; it includes the metamorphic solutions from within the earth as well as those that arise from surface action. Account must also be taken of such reversals of action as take material out of solution and return it to the solid state. All these must be considered, for they are all necessarily involved in the question of the age of the ocean. Some basal assumptions are unavoidable, and if we must deal with them, it is best to be frank and explicit about them. The necessary assumptions as to the early stages of the ocean are more or less speculative, but if we are to discuss the question of age at all, there is no occasion to be squeamish about that. It does not make the assumptions any less "speculative" to gloss over or shy at the fact that they are speculative or at least have speculative factors. Assumptions are least dangerous when explicitly recognized. They are even likely to be least speculative when the grounds on which they rest are carefully sifted, logically weighed, and made to throw such light as they may on the ques-

critical points was impracticable. The printed text gives somewhat more liberty and I have taken advantage of it to a limited extent and have recast this part of the text to accommodate it to this. I am greatly indebted to Dr. T. Wayland Vaughan, U. S. Geologist in Charge of Investigations on Sediments, for aid in securing documents and personal statements from the departments of our general government and from its officials engaged in investigations bearing on the question in hand.

tion in hand. We have no call to discuss the age of the ocean at all unless we are ready to be frank about the other end of its history. The crux of the issue lies there. We are all agreed about the age of this end.

The Two Types of Assumptions in Actual Use and their Radical Differences.[14]—Only two general types of assumptions require recog-

[14] The four estimates of the age of the ocean which were cited earlier and which give an average age of 95,000,000 years, with a range from 70,000,000 to 150,000,000 years, seem clearly to have been made on the basis of the inherited view of the origin of the earth. This assumes that all the material of the present hydrosphere, together with such substances of the present earth body as would be volatile at the temperature of molten rock, were held in the atmosphere which surrounded the supposed molten earth. The oceanic history is assumed to have begun when the waters from this primitive ocean-bearing atmosphere condensed upon the crust that had formed over the molten earth. The great influence which this view has had on geologic thought and the wide extent to which interpretations derived from it enter into various geologic concepts *not recognized as its offspring*, are chiefly due to the explicit teachings of the old masters who had clear cosmological conceptions and the courage of their convictions. Foremost of these among Americans was Dana, and as I once believed and taught this view but have become an apostate from it and the protagonist of another view, I trust that in following Dana's statment in the Fourth Edition of his Manual of Geology as a standard exposition I shall not be doing any injustice to the inherited view.

On the other hand, the only accretional view that has been carried out into any measure of detail is the planetesimal hypothesis. (The most recent statement of points pertinent to this discussion may be found in a series of articles entitled "Diatrophism and the Formative Processes," I. to XV., *Jour. Geol.*, Vols. XXI. (1913) to XXIX. (1921), particularly articles X. to XV.). To clear the air of needless fog let it be noted that this is not a speculation regarding the origin of the universe, or of the stars, or even of our sun. It is merely an endeavor to explain the singular dynamic properties of the earth and its fellow planets and their strange relations to the sun. It is merely a definite endeavor to solve a very definite problem. It started from an attempt to test the tenability of the inherited view of the atmosphere just outlined. The hypothesis that the atmosphere once held as vapor all the water of the ocean and much other volatile material, was framed before the nature of gases was known. The view seemed logical enough under the old notion of gases. Special reasons for testing it by the kinetic theory of gases arose out of the relations of the atmosphere to glaciation. The results of the test were very unfavorable. It seemed wholly improbable under the kinetic constitution of gases that a molten earth could hold so vast and active an atmosphere. This adverse result led to other tests of a more mechanical sort. These disclosed certain critical facts in the dynamics of the solar system which, while not al-

nition here, the one based on the view that the earth started as a molten globe, the other that it grew up slowly by the accession of solid particles. For the purposes of the present question it is not radically material how the molten globe arose, on the one hand, nor by what celestial mechanism the accretion took place, on the other, beyond the fact that the material of the ocean was supposed to be assembled as a vapor about a hot earth, in the one case, ready to begin work in full volume when cooling took place, while in the other case the waters came into action very gradually. Out of these basal differences, however, there arise some important contrasts in the modes of later action that are almost equally radical in their bearings on the evolution of the ocean, so that both the original and derivative differences need to be sharply in mind in considering the question of the earth's age.

1. On the one hand, it is assumed that the ocean was essentially uniform in volume throughout all the ages, and that the disintegration of the surface rocks, the inflow of solutions, and the content they carried were also essentially uniform. If these assumptions are correct, or if they hold true of a single leading element, as sodium, the present rate and content of inflow may be used as a divisor to ascertain the total time of inflow. This, however, is subject to the condition that there is no important reversal of action, or at least none that can not be adequately measured and discounted.

The alternative view assumes that the ocean grew to its present

together unknown, had not been adequately recognized as indispensable criteria in the interpretation of our planery system. In other words, the earth and its family have dynamic peculiarities that make the question of their origin a special one. *These hereditary peculiarities point the way to their interpretation.* The planetesimal hypothesis is simply the result of an attempt to follow these hereditary traits back to their parentage. It is as little as possible speculative, for it starts with mechanical properties which are rigorously determinate and which must be met by any hypothesis of genesis worthy of serious consideration. It follows these back to their probable origin in other known properties and natural actions so related to them as to be their probable sources. The method followed was only a phase of the standard practice of geologists in following the vestiges of a recorded event back to their most probable sources. If peculiar at all, it is merely in that the vestiges are dynamic. It ill becomes us to be squeamish about historical deductions from historical vestiges for there are plenty of people who regard geology a speculation from beginning to end and there is no present help for it.

volume very slowly from a small beginning, that the solutions came from three sources and were variable from the start, so that the whole history was very different from the preceding. The three sources of solutions were (1) the internal metamorphic action of waters entrapped in the growing accessions, (2) surficial action by the atmosphere and hydrosphere acting on the shell of the lithosphere, and (3) accessions of water-substance from the environing sphere under control of the sun—particularly accessions through the system of exchange between the ultra-atmosphere of the sun and the ultra-atmosphere of the earth.[13] The first source brought one type of solutions, the second another, and the third added water that was essentially fresh. Under this view it is obvious that until this complex of sources and variations has been worked out the present rate of accession has no claims to be regarded as a trustworthy divisor for ascertaining the total period of activity.

There are also two rather radically different methods of dealing with the geo-chemical evolution of the ocean. These are not necessarily connected with the preceding differences of view, but as a matter of fact they are closely associated with them.

The first of these—associated with the older cosmological view—takes for its start the concept of a universal crust acted upon from without by an atmosphere and hydrosphere, for its middle factor the streams, and for its end-products the sediments and the ocean. The matter in the sediments and in the ocean taken together are supposed to match the loss of the crust by decomposition and wear. Under this view any real failure to so match is a discrepancy to be accounted for. In the special problem in hand the sodium in the ocean, together with the sodium that remains in the sediments, should match the sodium once in the denuded rocks of the crust. So, also, the other elements of the crust should appear in due proportion in the sediments and the ocean. It is recognized in the cases of calcium, magnesium, potassium, silica, and other elements that there are reversals of action by which these elements go back into the solid state as new sediments, but it is held that sodium does not return to the solid state in the sediments in a similar chemico-physical cyclic way, to any appreciable

[13] "The Origin of the Earth," The University of Chicago Press (1916), pp. 19–21.

degree. Thus the sodium now in the ocean is held to represent the accumulation of all the geologic ages, and this total accumulation divided by the rate at which the present streams are carrying sodium down to the sea is held to give the age of the ocean, barring some corrections to be noted later. The crux of the whole issue of age lies in the validity of these concepts, particularly the irreversibility of the sodium.

The other view is far less simple. It looks upon the hydrosphere, of which the ocean is the chief concentration, as only the liquid phase of a solid-liquid-gaseous cycle through which the earth substances are passing. It is held that the earth is perpetually undergoing self-metamorphism in all its parts. This metamorphism takes place in a multitude of ways, each unit doing its part, in its own place, in its own way, and at its own rate. Each unit passes through its own cycles of liquid-solid-gaseous states according as its nature, its contacts, and conditions determine. Its career is wholly dependent on its own succession of conditions, and is only affected by what other units are doing under their conditions incidentally as it happens to come into working relations to them. The cycles that thus arise are so multitudinous and intricate that their correlation is a most formidable task which is scarcely yet fully appreciated; little more than a beginning has been made toward its accomplishment.

Under this view it is necessary to stress the fact that the simple solid-to-solution change from the rock to the ocean does not cover the whole evolution in the case of any substance. In most cases there are many cycles, some in parallel lines, some in succession. The content of the indurated rocks, on the one hand, and the content of the sediments and the ocean solutions on the other, are great features that have guiding value, but they are too general to cover with adequate accuracy the sub-cycles that make up the real history. The correlation of the whole is too largely conditioned by the number and speed of the constituent cycles to be successfully dealt with. It is especially affected by the reversals from the liquid to the solid state which take place during the passages from rock to soil, from soil to fresh-water solutions, or to colloids and turbid suspensions, and from these to the concentrated sea solutions in the borders of the sea. The

deductions drawn from such a complication of cycles differ in very important respects from the deductions drawn from a simple matching of the content of the igneous rocks with the content of the ocean solutions and the sediments.

Now, in respect to sodium, it is, of course, recognized that it returns to the solid state in less degree than potassium, magnesium, and calcium. It is held, nevertheless, on good evidence, that the sodium does return to the solid state in minor equilibrium degree and is recounted. The reactions involved are controlled by the law of mass action and the mutual effects of the constituents on one another. The reactions are particularly affected by the degrees of concentration, which are very low in the fresh-water solutions and quite high in the sea solutions. The trend of the reactions is toward equilibrium between the constituents, not toward any exclusive or monopolistic combination. Specifically, it is held that when the state of concentration favors the sodium, it will displace either potassium, magnesium, or calcium, and that such displacements take place as a standard feature in the processes of disintegration and solution, though only in an appropriate minor degree.

Let us now turn to such determinations as are available for testing the validity of these contrasted interpretations.

Discrepancies in the Matching of Igneous Rocks with Sediments and Solutions.—The differences between the content of the igneous rocks and that of the sediments and the salts of the ocean have been put in definite form by Leith and Mead.[16] Comparing first the igneous rocks with the sediments, they find the following excesses and deficiencies: (1) a *deficiency* of 3.1 per cent. in iron; (2) a *deficiency* of 26 per cent. in magnesium; (3) an *excess* of 32 per cent. in calcium; (4) a *deficiency* of 64 per cent. in sodium; and (5) an *excess* of 2 per cent. in potassium. If the corresponding constituents in the ocean are added to these severally, some of the discrepancies will be lessened, while others will be increased; the discrepancies do not disappear, though they are somewhat mitigated.

2. It is recognized on all hands that the land waters vary greatly according to the nature of the drainage area from which they are

[16] C. K. Leith and W. J. Mead, "Metamorphic Geology" (1915), p. 69 *et seq.*, particularly pp. 83–88.

derived. In some districts they consist largely of carbonates, or of sulphates; in others of chlorides, or of silicates; while the degree of dominance varies greatly within each class. The solutions of the ocean, however, are not identical with any of these, nor with a simple mixture of them; the ocean solutions are dominantly chlorides, but constitute a combination which is quite distinctive. This implies that, instead of a theoretical mixture of the land waters, an effective chemico-physical reorganization takes place, a liquid metamorphism of the heterogeneous land waters and their content into the homogeneous sea solution and its sediments. This is in a measure recognized, but the recognition is inadequate if the change is regarded simply as a liquid metamorphism. There is a neglected solid factor in the form of silts and clays that is of critical importance. The usual comparison is really between the *clear* waters of the streams—which are mainly the outflowing *ground* waters of the land—and the sea waters. The *run-off* and its contents—the wash-waters of the land and their burden of mud—are neglected. But it is this run-off water with its mud and the colloids that go with it which carry the larger part of the acid radicals of the soil from which the basic radicals were leached. The reunion of these acids with the alkalies in the border of the ocean constitutes a critical part of the metamorphism which gives rise to the ocean solutions and sediments. We will return to this presently.

3. *The Larger Part of the Solutions now Flowing into the Ocean Comes from the Sediments; the Lesser Part from the Igneous Rocks.*—This becomes the more suggestive when it is noted that the sediments have been worked over repeatedly in some notable part; some small part, perhaps hundreds of times; some larger part, scores of times; while some other large part perhaps has not been worked more than once, unless we count in the many times most material is handled in going from the parent rock to the ocean. That the sediments should still be able to yield saline solutions to the observed extent raises a vital question into which it is necessary to inquire before assuming the practical non-reversibility of the sodium solutions. It is already well recognized that a part of this sustained productiveness is due to sea-winds which carry salt inland from the

ocean. This salt is thus counted as many times as it is carried back. An endeavor has been made to estimate and make allowance for this by taking the increase of salt solutions near the ocean as a criterion. It has also been recognized that salt solutions are entrapped in the pores of the sediments as they are laid down under the sea, and that when the beds are afterwards raised above the sea-level these solutions are drained into the streams and counted again as salts from the land. The amount of duplication involved in this depends on the ability of the rocks to hold salt water mechanically in their pores, and correction has been sought by estimating their porosity and discounting for it. Sandstones usually have the highest porosity and limestones come next, while shales are relatively close-textured and impervious, but still the shales are exceptionally productive. So, also, it has been recognized that beds of rock-salt occur in the stratified series, but these are held to be relatively unimportant. So still further some particles of the original rock may remain undisintegrated; so, too, fresh particles may be cut away from exposed rocks by wind blast and widely though sparsely distributed. But when the modifying effects of all these have been recognized and discounted, there still remains a serious source of double counting of sodium which we must consider presently.

4. The ratio of chlorine to sodium is a crucial matter, recognized but not sufficiently emphasized. Inspection of the drainage from regions of igneous rocks shows that *the chlorine is relatively low and the sodium relatively high* compared with the ratio of these elements in the ocean, which is about 1.8 chlorine to 1 sodium. The relative deficiency of chlorine in the drainage from the very rocks that are assumed to be the ultimate source of the salt solutions raises a fundamental issue.

5. In view of this, let us make our inspection as sweeping as possible. Let us compare the ratio of sodium to chlorine in the ocean with the ratio found in the average igneous rock of the whole "crust." The latest and most authoritative determination of the chemical composition of the igneous rocks is that of Clarke and Washington, which gives the mean sodium content as 2.83 and that of chlorine as 0.096.[17]

[17] Frank W. Clarke and Henry S. Washington, U. S. Geol. Surv. and Geophys. Lab., Carnegie Institution of Washington, "The Average Compo-

From this it appears that the mean per cent. of sodium in average igneous rock is *about thirty times as great as their content of chlorine.* This is a large difference, but it does not represent the full discrepancy, for the chlorine in the ocean exceeds the sodium in about the proportion of 1.8 to 1. Taking this into account, the discrepancy rises to somewhat above *50 to 1.* This is a formidable discrepancy. How is it to be met on the assumption that the sodium in solution is not reconverted into sodium solids, but remains in perpetual solution? The dilemma is not much relieved by reckoning in the sediments and the ocean salts, for Clarke and Washington also give[18] the ratio of sodium to chlorine when the atmosphere and hydrosphere are reckoned in with the outer ten miles of the lithosphere. The discrepancy, corrected for actual oceanic proportions, is even then nearly 20 to 1. Quite naturally volcanoes have been thought to be a source of excess of chlorine, but any contribution from the volcanoes is covered by this inclusion of the whole atmosphere and hydrosphere in the average. Besides, the later studies of volcanic gases do not sustain the earlier views that they contained a specially high content of chlorine.[19] The observed differences between the sodium and the chlorine appear to have grown mainly out of the normal processes of cyclic change when these are viewed in their largest aspects. If the sodium returns to the solid state in due (though lesser) proportion to the potassium, magnesium, calcium, and chlorine, as these constituents are found mixed in the solutes and the sediments, there is no necessary discrepancy in these great differences. The discrepancy is constructive and is imposed by the assumption that the sodium does not take its proportional part in cyclic action. Under the alternative interpretation, the amounts of the several elements present in the ocean are primarily functions of their own cyclic histories; their proportions

sition of Igneous Rocks," *Proc. Nat. Acad. of Sci.*, Vol. 8, No. 3 (May, 1922), pp. 108–13. In the paper as read at the Symposium I used the then latest and most authoritative figures, viz.: those of H. S. Washington, "The Chemistry of the Earth's Crust," *Jour. Franklin Inst.*, in which the sodium was given as 2.85 and the chlorine as 0.055.

[18] Ibid., p. 114.

[19] E. T. Allen, "Chemical Aspects of Vulcanism with a Collection of the Analyses of Volcanic Gases." Papers from the Geophysical Laboratory, Carnegie Institution of Washington, No. 440 (1922), pp. 1–80.

are not predetermined solely by the composition of the igneous rocks now at the surface, but rather by the relations of their own solvent to their non-solvent natures under the conditions of their long complex history. Specifically, in the case of sodium and chlorine, the observed ratio merely means that the solution stages of sodium compared with its solid stages are much inferior to those of chlorine, just as those of potassium are much inferior to those of sodium, and so on through the list. But, however cogent this may be, definite evidence that sodium does enter freely into the cyclic processes, in due proportion to the action of its associates, however inferior the proportion may be, will naturally be demanded. Allusion has already been made to a neglected factor. Let us turn to that.

The Mud Cycle Actuated by the Surface Floods.—Familiar as this is in many respects, it has perhaps received less critical geochemical study than almost any other common feature of nature with which we are directly, not to say unpleasantly, brought into contact. The agricultural chemists have naturally been preoccupied with those elements of the soils that serve as plant food, the students of hygiene and domestic science, with waters suitable for drinking and culinary purposes, and the geologic chemists with the organic extracts and precipitates that form the limestone, dolomites, and siliceous beds. The mud factor of the surface wash has been neglected. And yet the muds (later shales) comprise much the largest part of the solid residue of disintegration. This solid disaggregated residue and the colloids associated with it are separated from the true solutions in large measure at the very start on their long journey to the sea. The true solutions are largely formed by waters that descend through the soils into the underlying formations and thus form the ground waters which pass by springs and seeps into the streams, giving them their steady supply of clear water. This is the water chiefly analyzed and taken into account in reckoning the material borne by the stream to the ocean. The solid residue, the clays, silts, and sands, however, are only slightly removed by the gentler rains which soak into the ground. They are carried down to sea chiefly by the floods following heavy storms, or by the thaw-waters of winter snows which form the spring freshets, or by flood stages from any cause. The turbid mat-

ter of these muddy waters contains a large part of the acid radicals with which the basic radicals of the true solutions were united in the parent rock and in the soils. While it is known that the muddy waters contain hydrous silicates of alumina and iron, partly colloidal and partly non-colloidal, together with finely divided siliceous silts and colloidal silica, full and exact information is lacking. Dr. Collins says that "the dissemination of silica in natural waters, particularly turbid waters, is one of the least accurately known of the determinations of substances present in appreciable quantities."[20] He adds that even "the exact state of the silica present in a perfectly clear water is usually not known. It may be colloidal or it may be present as a silicate radical." In addition to this—or perhaps the cause of it—investigation is embarrassed at the inland end of the cycle by the fleeting and irregular nature of the freshet stage, and by the rapid and intricate changes within the soils. The changes in the soil are so rapid in certain respects that F. H. King found it important to make his determinations of water-soluble solutions by means of an improvised laboratory in the field so that determinations might be made as promptly as possible after the sample tube had taken the soil from its natural relations.[21] At the sea end of the cycle the recombinations of the acid radicals with the basic radicals seems to take place chiefly at the base of the turbid water as it is carried out over the concentrated sea solutions and diffuses into them. Before the acid radicals reach the bottom the reversing phase of the cycle has probably ended and a new cycle begun under sub-oceanic conditions. The experimental evidence in support of this conclusion is buried in a great mass of literature which relates primarily to other elements, particularly the elements that form plant foods, such as potassium, phosphorus, etc., and those that form precipitates such as calcium and magnesium carbonates, but when these scattered data are gathered together their combined import is sufficient to make clear the essentials of what happens.[22]

[20] W. E. Collins, Chief of the Quality of Water Division, U. S. Geol. Surv. Personal communication.

[21] Bull. 26, U. S. Dept. Agri. (1905), pp. 26–27.

[22] The following are among the more important early investigations:

Way, *Jour. Roy. Agri. Soc.*, Vol. 11 (1850), pp. 313–79; Vol. 13 (1852), pp. 123–43; Vol. 15 (1854), p. 491.

The present status of knowledge and opinion is summarized by the following quotations.

Dr. Truog writes:[23]

The minerals or salts in soils consist largely of silicates. On weathering the bases are removed from the silicates, leaving acid residues or acid silicates. These acid silicates will react with salts like KCl and NaCl and remove the base and leave HCl in solution. When soil is treated with equal molecular strengths of these two solutions, the potassium is removed to a greater extent than the sodium. This is due to the fact that the potassium forms more insoluble compounds with the acid silicates than the sodium. Furthermore, silicates which have not had their bases removed will also react with these salts and exchange bases with them. For example, potassium chloride will react with an insoluble sodium silicate in which reaction the potassium replaces the sodium and the sodium is left in solution as soluble sodium chloride. If an insoluble potassium silicate were treated with a solution of sodium chloride some of the sodium would replace the potassium and some potassium would thus go into solution as the soluble chloride. This, however, would not proceed to as great an extent as the previous reaction, since the potassium forms a more insoluble silicate than sodium. In reading some of the literature on this subject one may get the impression that sodium is not retained by soils like the potassium, but this is really not the case; the action is merely relative. The potassium is retained to a greater extent simply because it forms more insoluble compounds with the soils.

Dr. Milton Whitney[24] writes:

The investigations of this Bureau[25] show that the absorptive power of a

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Eichhorn, *Pogg.-An.*, Vol. 125 (1854), p. 126.

Voelcker, *Jour. Roy. Agri. Soc.*, 2d series, Vol. I., pp. 289–316.

Kullenberg, Hoffman's "Jahres bericht der Agrikultur Chemie," Vol. 8 (1865), p. 15.

Lemberg, *Seitschr. deutsch. Geol. Gesell.*, Vol. 29 (1877), p. 483.

[23] E. Truog, Soil Chemist, Dept. of Soils of the College of Agriculture, University of Wisconsin. Personal communication.

[24] Milton Whitney, Chief Bureau of Soils, U. S. Department of Agriculture. Personal communication.

[25] These include numerous publications containing many analyses as well as special discussions, but as in all agricultural publications, the constituents that most concern plant life receive most attention and data relative to sodium is incidental. The following may be cited:

Cameron, F. K., and Bell, J. M., "The Mineral Constituents of the Soil Solution," U. S. Dept. Agr., Bur. Soils, Bull. 30, 1905.

Cameron, F. K., and Patten, H. E., "The Distribution of Solute between Water and Soil," *Jour. of Phys. Chem.*, Vol. II., pp. 581–93. 1907.

Patten, H. E., "Some Surface Factors Affecting Distribution," *Trans. Amer. Electrochem. Soc.*, Vol. 10, pp. 67–74, 1906.

Patten, H. E., and Gallagher, F. E., "Absorption of Vapors and Gases by Soils," U. S. Dept. Agr., Bur. Soils, Bull. 51, 1908.

soil resides almost wholly in the ultra clay or the colloidal material in the soil. This ultra clay is mainly a hydrous silicate of alumina and iron, with hydrated oxides of iron and probably alumina and absorbed calcium, magnesium, sodium and potassium. It is of a colloidal nature, and can be separated from the soil in the form of minute droplets in dilute colloidal solutions which form into colloidal aggregates when the concentration is somewhat over one gram per 1000 c.c.

The chemical analysis of the soil colloids which we have separated shows considerable amounts of lime, potash, soda and other material which we believe to be absorbed in colloidal condition. We believe there is a distribution between the amount so absorbed and the concentration of the non-colloidal part of the solution. We believe also that the absorption of any one of these constituents such as potassium will be influenced by the presence of other salts such as sodium or calcium. Under all stable conditions there will be an equilibrium between the amount absorbed and the concentration of the surrounding liquid. Sodium chloride lowers the absorption of potassium chloride and calcium salts lower the absorption of potassium chloride. In general soils and the colloids obtained therefrom absorb the basic ions much more readily than they absorb the acid ions.

According to Clarke the earth's crust contains 3.28 per cent. of Na_2O and 2.96 per cent. of K_2O. Thirty soils and the colloids obtained from the same collected by this Bureau contained in the soil 1.59 per cent. of K_2O and 1.45 per cent. in the colloids. The soils contained on the average 0.77 per cent. of Na_2O and 0.29 per cent. in the colloids. These figures show very clearly the greater power possessed by the soil colloids to absorb and to hold back potash than they have for sodium.

A diffusion experiment with a soil colloid lasting over two months in which large volumes of distilled water were allowed to act showed a loss of 25 per cent. of total K_2O and over 95 per cent. of the total Na_2O.

There is no question that the soil colloids are able to absorb NaCl. This is shown by the ancient experiments of making sea water drinkable by filtering through soil filters.

Data and references examined show that under conditions of leaching by rain water where equilibrium conditions are changed potassium is largely retained by the soil but sodium is largely leached out.

In the presence of much NaCl, as is found in sea water, ocean shore deposits would undoubtedly absorb considerable NaCl up to the point where the colloids were in equilibrium with the sea water. If the material were then formed into a shale and elevated to land areas the induration would presumably destroy the colloidal properties leaving the NaCl free from its colloidal entanglements and with the change of the solvent from sea water to rain

Patten, H. E., and Waggaman, W. H., "Absorption by Soils," U. S. Dept. Agr., Bur. Soils, Bull. 52, 1908.

Schreiner, Oswald, and Failyer, G. H., "The Absorption of Phosphates and Potassium by Soils," U. S. Dept. Agr., Bur. Soils, Bull. 32, 1906.

Parker, E. G., "Selective Adsorption by Soils," *Jour. of Agr. Research*, Dept. Agr., Vol. I., No. 3 (Dec. 10, 1913).

water equilibrium conditions would be expected to remove readily a considerable amount of the NaCl while the K_2O would be largely retained on the weathering of the shale and the reformation of colloids resulting therefrom.

From these authoritative statements of present knowledge and opinion; from the early experiments of Way, Eichhorn, Kullenberg, Voelcker, Lemberg, and others, in mingling soils with salt solutions and in passing salt solutions through soils, and from many intermediate experiments cited by Sullivan,[26] there is left little ground for doubt that when the acid radicals previously separated from the basic radicals under conditions of very low concentration, again meet basic radicals under conditions of high concentration in the ocean off the mouths of the streams, reunion takes place in equilibrium proportions. The experiments of Lemberg are particularly instructive on this point. He treated potassium-aluminum silicates with sodium-chloride solutions of different degrees of concentration, and after thoroughly washing the solid material so treated found *that potassium had been replaced by sodium* in increased quantities as the concentration of the sodium solution was increased. Complete replacement of the potassium by the sodium did not take place, but only replacement to the degree required by the law of equilibrium. Now if, in addition to laboratory results, we recall that in former times salt water was freshened for use by passing it through soil, the periodic flooding of the border waters of the ocean by soil wash from the lands may be looked upon as a natural process of salt-water freshening. As there was wash from all the lands, and as the shales formed from the wash products are known to make up much the largest part of the sediments, the process was really one of great magnitude.

As the recombinations are divided among the constituents in accordance with the law of equilibrium, the sodium gets a smaller share than the potassium, but it gets a share. From the imperfect evidence one may guess that the sodium recombines to a third or a fourth of the extent of the potassium, but whether more or less than this proportion, it seems clear that enough sodium reunites with the acid radicals in their solid state to vitiate the use of sodium solutions as a criterion of age. This is as far as the present issue requires me

[26] Eugene C. Sullivan, "The Interaction between Minerals and Water Solutions with Special Reference to Geologic Phenomena," Bull. No. 312, U. S. Geol. Surv. (1907), pp. 7–62.

to go. Doubtless other cycles follow in the sea and in the sedimentary beds, particularly when deformations take place or igneous and metamorphic actions follow, but we need not dwell on these.

The Cycles of Chlorine.—The climax of the solvent actions that enrich the sea is reached in the cycles of chlorine, but only a passing word can be given to these. The tenor of experiments with soils indicates that chlorine remains more persistently in solution than the sodium and associated substances. As the cycles of each substance spring from its own nature and the conditions it encounters, the very high preponderance of chlorine over sodium in the ocean finds its chief explanation in this more persistent solubility. Its proportion in average rock is only a conditioning factor and is not the chief controlling influence. When compared with sodium, which is much more abundant in the igneous rocks and indeed in the whole substance of the outer ten or twenty miles of the earth shell, atmosphere, and hydrosphere included,[27] the logical conclusion is that the cycles of chlorine have always had a much larger liquid phase than those of sodium, and that this has been cumulative through the ages. Chlorine is better fitted than sodium to be used as a criterion of age, but even in this last case there are formidable difficulties. Both sodium and chlorine and all the other constituents, as already noted, have their own histories which are difficult to disentangle. As Roger Bell neatly puts it: "There are as many histories to be written about the waters as there are kinds of sediment."[28] There would be an ocean highly charged with chlorides if there were no sodium in the earth at all. So there would be an ocean highly charged with sodium solutions if there were no chlorine in the earth. The status of the ocean at any time is simply the equation of the solution phases of the antecedent cycles of its constituents, all of which have passed through long, complex, more or less individual histories. In the tedious work of their disentanglement the older and simpler geo-chemical notions will not answer; the newer principles of chemistry, physics, and geology are indispensable.

Conclusions.—Our finding, then, in respect to the age of the earth from the geological viewpoint is this:

[27] Clarke and Washington, ibid., p. 114.

[28] Personal communication.

1. Estimates of time based on the well-preserved series of geological sediments will, when adequately corrected, probably fall into harmony with the revised deductions from paleontology, radio-activity, and astronomy, so far as these cover the same ground.

2. The distorted and metamorphosed terranes below the well-preserved series of sediments do not disclose the starting point of sedimentation. The sediments can therefore give no verdict on the *total* age of the earth; they are great enough, however, to show that the earth is very old.

3. The science of hydrogeology, of which oceanology is only a part, is not yet ready to render a verdict; it has more need of a court of inquiry than a place on the witness stand.

UNIVERSITY OF CHICAGO.

THE AGE OF THE EARTH FROM THE PALEONTOLOGICAL VIEWPOINT.

By JOHN M. CLARKE.

(*Read April 22, 1922.*)

It falls to me to consider this knotty problem on the basis of the biological evidence alone, in so far as it is possible to disentangle this from its almost inevitable complication with geological accompaniments. In saying biological I mean, of course, biology with the time element generously admitted; that is, not the biology of the instant, the present, but the long biological panorama leading up to the present. Thus I am in a different case to some of my colleagues, for I presume it safe to say that life can have come into being only as a secondary potency in the evolution of force. Just what I mean is that the combination or interaction of physical energies of different categories did not produce the form of energy we designate as life till after a very long chapter of the earth's planetary history had been written. I may as well frankly say at the beginning that there can be little hope of arriving either at a reliable or an approximate conclusion as to the age of the earth through this paleontological channel, unless the study of the chronological development of life may in some way afford a measure of the rate of vital processes and thus the measure of some short span or infinitesimal fraction of earth history. This is a shadowy road and this presentation must resolve itself into consideration of such evidences as there may be for time-requisites in the consummation of evolutionary biological procedures, whether in gross or in detail. The bare statement of this fact in such vague form must carry with it an indication of the grave uncertainty of the results except to minds of the fourth dimension. I am not convinced that it is within the power, now or ever, of even the most refined understanding of paleontology, to accomplish this and establish such standards of measurements. Nor am I at all confident that the attempts which have been made to establish such rates of procedure

could justify the great labor they have exacted, were it not for the important accessory facts they have elicited.

There seems to be no effective reason or very good philosophy in declaring, as some of our writers have been wont to do, that all life is one life. We seem to have really established the polyphylogeny of several races not only in the lower phyla of animals and plants, but among the vertebrates, and in the thought of competent authority, even to the inclusion of man, and we assign these like products to a differently governed and directed inheritance emanating from fixed points in evolutionary history. This is an enlarging point of view in the interpretation of past life, and admitting its general effectiveness we can conceive and can justify a concurrence of physical energies which need not, and indeed should not logically, be restricted to some single outburst and some single definite moment in earth history. This intimation is that life itself may be polygenetic, though we would not have it interpreted as applying to the reiterative appearance of inceptive life through the ages, which is an old conception that still awaits its justification; it is rather only the precise implication of a terrestrial condition so controlled that by the intersection of the requisite forces life came into being at the *points* rather than at the point of such intersection—a crude way of stating it, perhaps, but it is an intimation of my meaning.

When we gaze upon some of Walcott's Burgess Pass fossils, see the extraordinary intricacy of their anatomy, as, for example, the crustacean Burgessia, with not merely the delicately toughened parts of its exterior, but the evidence of internal organs of great refinement, the lobulation and venation of renal organs; and, in the trilobite Neolenus, the multiplex delicacy of gills and swimming or walking organs, the effective impression is that, as between such creatures and their nearest allies and perhaps their offshoots of to-day, there is no difference in degree of specialization of structure, no progress in perfection of organic function. Indeed, we may even go further; modern allies of these creatures are in close straits of adjustment to their own physical surroundings, which are too often indicative of the surrender of progress, and to this I shall again make reference. But the Walcott fossils are from the Middle Cambrian, almost the

oldest term in the whole long series of rocks in which life has been well preserved, and we here, in this year 1922 of the Christian era, are unable to find that any progress has been made in the structure of these creatures or along the direct line of their development and succession. Their successors in time and place have adjusted, readjusted, adapted, and readapted themselves without having produced a creature of their tribes which can be called a more intricate or a more perfect mechanism.

And yet what has gone on in that vast interval of time from then to now? The successive derivation of all intermediate types of life have come into being. The trilobite Neolenus, from the viewpoint of the paleontologist, stands for a tremendous conception of the vastness of time behind it. This inconspicuous thing, standing back behind us in the dim days of the Cambrian, stripped bare now by the arduous labors of its discoverer, reveals a creature so highly specialized that it must have commanded uncountable ages for its production by any such process of organic development as that to which we paleontologists make our allegiance. The problem behind the Neolenus is that of having developed out of the unicellular expression of life, under favoring physical conditions and directive impulse, this intricate and closely functioning organism. How long did it take? I would like to put the problem to the experimental biologist: Given an organism with a full equipment of motor and sensory nerves and an elaborated digestive tract, with specific organs of circulation, reproduction, and of waste—is the distance greater from that starting point to the specialized creatures of the present, ourselves if you will, or from the nuclear cell (which we must hold to be not alone the seat but the radial point of life) up to that marvelously specialized creature? Starts are slow, progress to be secure must be deliberate, the momentum of the impulse must be acquired gradually, the passage from a protozoan to a metazoan means the crossing of a deep moat, the climbing of a high wall. But the directive once acquired, then matters may go forward with acceleration. On the basis, then, of the structure of this ancient trilobite alone, it is safe and probably necessary to answer that Neolenus was farther away from the beginning of life, very, very much farther away, in the highest probability, than we of to-day are from Neolenus.

This is a relative expression, but we can not be more concrete. The Walcott discoveries have lifted the veil from a scene in the panorama of life that was barely guessed before. In our previous general understanding there was, in the still earlier faunas, a group of creatures believed to be of simple structure and lowly place in the category of life; it was thought that with these simple things the caravan of life had got under way for its journey through the ages; and now we are compelled to believe that the journey was half over when this caravan first came under our eye.

It is not my part to make a review of statements and calculations of earth age based on the rates of sedimentation from the Cambrian time on to the present; but whatever these are, they may, from the biological point of view, reasonably be doubled and then increased by some improper fraction, if we are to reach a competent expression of the duration of the life-day of extinct species—the zoëhemera, as I termed it many years ago, and of the sum of these which go to make a fraction of earth history.

It has not been the practice of students of evolutionary paleontology to raise the question as to whether there were time enough available for the production of the succession of results which pass under their eye. Such an attitude would of itself be highly unphilosophical, and only a natural inquisitiveness or curiosity quite unessential to the real philosophy of the succession and purpose of life has led to the occasional investigation as to the possible time-rate of evolutionary processes under historic and under natural conditions. We are not the makers, but the users, of time. There have been stages in the history of our science when we have been treated gingerly by astronomers and physicists in the allotment of time, but now that our colleagues in celestial mechanics are heaping upon us their munificence in the prescription of this heavenly commodity, we are content; and the interpreters of radio-chemistry—we thank them for giving us what we already had. There is time enough. So much, indeed, that to absorb a needful share of it into the philosophy of the evolution of life actually requires of us a revision of our conceptions.

I should, I think, take passing notice of the fact that the problem as to how species have originated, one from another, with or without

the help of mutations, variations, or variants, the problem of the factors which have controlled their production, does not belong to paleontology. Bateson, speaking recently at Toronto, has expressed the conviction that after the nearly three quarters century since the publication of Darwin's Origin of Species we are still in doubt and darkness as to the causes of the origin of species. Incautious as it seems, that expression would still be a hopeful one if it means that in this relatively brief period the study of this theme, stimulated by Darwin, has led to the elimination of an extensive array of supposed factors, so that if the buried treasure, if it is really the treasure he has thought, has not yet been found, at least some of the brush has been cleared away from about the place where it lies hid. Both laborers, those in the field of living nature and those delving among the past creation, see the engrossing fact of evolution, but see it out of different eyes; the former perhaps as one would see a vast throng gathered together to acclaim a momentous event, a great victory or a high armistice; the latter as an endless army marching by, its vanguard already out of sight in the mists of the horizon, stragglers along the way falling back or giving up in hopelessness, while the interminable procession ever emerges out of the shadow.

Once upon a time, when Walcott was first bringing out his wonderfully specialized Cambrian fossils from the Burgess shale, I said to the discoverer in a jocular way, "Keep on and you may find the remains of a Cambrian man." In the recent address referred to, Bateson ventures more solemnly into this field. "It has been asked [I am quoting] how do you know, for instance, that there were no mammals in paleozoic times? May there not have been mammals somewhere on the earth though no vestige of them has come down to us? We may feel confident there were no mammals then, but are we sure? In very ancient rocks most of the great orders of animals are represented. The absence of others might by no great stress of imagination be ascribed to accidental circumstances." Considering that these remarks were made in the presence of a great body of scientific men, among whom were paleontologists, I fear the speaker neglected to do what he should have done and as Artemas Ward was wont to do in like case, for in no evidence from any quarter, whether

it be of biology, geography, geology, meteorology, oceanography, or psychology, is there the slightest justification for seriously embalming such a fancy in a scientific address and sending it abroad in the world for the daws to peck at.

We must fasten our gaze upon such impressive evidence as can now be adduced of the duration of time required in the attainment of organic specializations, and let me supplement those I have given by others taken from the plant world. Casting up the evidences that have been adduced by paleontologists and paleobotanists, I think the footings show very positively a large balance of argument in favor of the great conception that the life of the land has emerged from the sea. I believe it may be said, on behalf of paleontologists generally and their broader deductions, that these are happy in the harmony of their conclusions in this matter after having experimented with and checked up alternate conceptions.

The broader lines of evolutionary derivation and the best weighed deductive propositions seem to intimate a convergence of the life lines back to the sea and a radiation from it. The inception of life was the most solemn moment in the history of the Universe. We invite certain astronomers to refrain from further speculations and presumptions as to life in other worlds, and followers of Arrhenius from pursuing life spores through interplanetary space. These notions seem to be very exciting to the emotional public and there is indeed no shred of evidence of these things, no matter what physical conditions may be predicated of other worlds than this. So far as the evidence of outstanding facts and major probabilities goes, Life is confined to the Earth. Into this solemn event, the birth of life, the interaction of the forces requisite to emergence, we shall not here attempt to pry. We look back, then, to a primitive period of life in the sea, the Plankton epoch, the place and stage of life's emergence, the surface life; followed by a Benthic epoch, the secondary stage of development in which the living forms had found the shallower sea bottoms and thereupon began their adaptations and more rapid evolution.

I shall now borrow freely the brilliant conceptions of Church, the British paleobotanist, as to the procedure among the plants thence-

forward from the sea to the land, an act which implies time in impressive measures and yet an act which we know has reversed itself in later geological times, at least among the animals, with nostalgic energy and must again and again have shown a like reversion in both the animal and in the plant world. We see suggestions of these reversionary movements among the Amphibia and the Mollusca and many Mammalia, and it seems highly probable that a more exact knowledge of extinct life will establish these suggestions and awaken others.

The Plankton epoch, says Church, gave rise to the first encysted flagellate plants which, under conditions of the Benthon, developed multicellular thallus, tissues and organs of special function and a reproductive mechanism contrived so as to minimize waste. Then followed the Epoch of the Land Flora brought on by the transmigration of highly developed algæ which in fact "appear to have been more highly organized than any single algal type at present known to exist in the sea." "The algæ of transmigration may be . . . said to have combined the best features of the known great conventional series of marine phytobenthon." "The origins of all the main successful adaptations of the land are to be traced down to the benthic phase of the sea." In this impressive statement we are confronted by the quality of the plant life at its emergence from the sea.

Now as to the period of its emergence, of foremost importance to our present consideration; Thomas C. Chamberlin in 1913 directed attention to the fact that the Precambrian rock complex is divided into earlier and later stages on the basis of the degree of disintegration of the exposed rock surface. In the lower division there is an immature disintegration which implies partial decomposition, but the mature disintegration of the later division implies, he says, "some restraining agency that held the rock in place while the slow weathering completed its work." "This view favors the existence of a vegetal covering of the land as far back as this period."

Church, therefore, has a well-found argument when in the presence of this fact of precambrian weathering he intimates that it was with the uplifting and exposure of the primary rock to the air that

"the marine organism was brought into direct association with atmospheric air and subaërial environment to mark out new lines of progression to still higher and more strenuous forms of land life, though these are again necessarily expressed in terms of preceding organization and mechanism." The point to be made here is that with the earliest lifting of land from the sea, benthic algæ of advanced structure, "the remarkable algæ of transmigration," as he has characterized them, got their foothold on the land. "The Evolution of the Land Flora was a phase of transmigration in *situ*" and did not involve a preliminary landward migration by the way of fresh water, "the biological factors being exposure to more or less desiccation and the removal of the food solution." "The few races that survived only did so by pressing to the utmost any principles of economy in reproductive output that they may have previously initiated," such as oögamy and fertilization in *situ*.

The picture presented by this line of carefully founded reasoning is even more impressive in its demands upon time than the argument we have presented from animal life. It is summed up thus: Plants of complex organization and function—deductively of higher organization than can be to-day found among the algæ—had worked out their attainments before their arrival on the land, and probably this organic achievement, not surpassed in the seas of to-day, was accomplished at a stage in earth history long before the Cambrian Epoch brought with it the tangible evidence of the complex animals. The argument from the plants is more highly deductive than that from animals, but its steps are logically taken from effect to cause, and in its presence we must stand uncovered at the inconceivable lapses of earth-time through which these transmigrant plants were slowly working out their organization in the waters when there was no permanent land—a period of time which must have been longer than all time that has passed since the emergence of the Laurentian or the basal rock complex of the great shields of the earth.

II.

If we are prepared to concede the steadily increasing weight of evidence of the polyphyletic origin of genera which recent researches

have indicated for so many different groups of life, and can compel our conception to grasp the duration of the vast unrecorded past of life-history, there remains another phase of the paleontological record which in part emphasizes and in part serves as a check on this conception. It has fallen to me to study the earliest recorded expressions of dependent life—that is, the beginnings, so far as we can find them, of such consociations of animals as we are wont to designate as parasitic, mutualistic, and symbiotic, wherein one creation has depended upon or adjusted itself to the life functions or habits of another, or has sought mechanical protection at the cost of its own locomotive independence. Two very obvious facts seem to stand out as a result of these inquiries: (1) That these interdependent conditions with which the living world is rife to-day, in passing backward to the early stages of Paleozoic time, become palpably fewer; indeed, while such conditions are well marked in some groups and common in others during the middle and later Paleozoic, they are very unusual in the earlier stages and in the Cambrian fauna are little more than suggested. (2) This dependent state seems with reasonable clarity to be resolvable into an original loss of locomotive independence, a willingness to be fed rather than to feed, an adaptation to an easier mode of life. The commanding percentage of the Cambrian fauna belongs to groups against which the charge of surrender of locomotive independence can hardly be laid, though inclusive of groups of animals which in later stages did become infected with the loss of independence, but still in a capital sense embraced those whose independent living was unimpaired.

These considerations I have analyzed elsewhere in some detail and their significance is this—that the degeneration of life (for dependence of necessity implies degeneration of physiology) has been a process attendant upon and of course influencing evolution, but apparently limited in its effects to that part of the procession of life which comes under our actual observation; that is, since the days of the free and independent faunas of the Cambrian. If this is an approximation to the truth, as we believe it to be, then in a broad sense the real vigor of life, which established the major branches and laid down the plan of all future ages, was dominant in its purity in

the ages before the beginning of the life record in the rocks. How often the student of the past of the earth has exclaimed at the wonder that Man came through to his excellence, in a world permeated with ever-increasing conditions of degeneration.

III.

With such propositions as the foregoing we are confronted by an impressive requirement of time necessary to the development of life on the earth. It is a requirement that seems to roll back and ever backward into the undifferentiated ages of our planetary history. It is a magnitude that takes on proportions before which the outstanding estimates of time based on processes of rock building would seem to dwindle, and it partakes more and more of the magnitudes in which the radiologist has been wont to speak. The question for us now is whether our present knowledge affords any basis for an estimate or calculation of this time or any part thereof into a concrete expression. If it were possible to estimate by any or all approaches, the length of the life of a single extinct species in any part of the world, there would then lie a possibility of determining what fraction this given quantity might be of the whole. For more than two generations the evidence has been sought, paleontologists endeavoring first to establish the endurance of a given or index species as the basis of a geologic or stratigraphic element—a zone.

Into the discussion of the Zone—its meaning in time and space—has entered a very long list of eminent names in the science. The Zone has been looked upon as a sedimentary element in which a datum species slowly coming to its acme suddenly culminates and abruptly disappears; as such sedimentary unit in which not a species, but a mutation, or an entire fauna rises and falls. To Oppel the Zone was a space-unit. Buckman has embodied the time conception of the Zone in the word *hemera*. The double combination of time and space makes a *biozone*. The time unit has also been termed *sæculum* by Jukes-Brown, *moment* and *phase* by the International Geological Congress. In the recent summary of these expressions and their interpretations as given by Diener, in order to determine a proper basis for his discussion, he employs the term Zone for the spatial, that is

horizontal and vertical distribution of a fauna, whose time is a Moment.

The whole interpretation of these conceptions centers upon the origin and endurance of a mutation, which in the proper paleontological sense is a departure from a recognized species toward and into a unit which, by determinate action of the genes producing variation, will become another species. That is to say, the mutation is a clearly recognizable entity in paleontology, is the bridge crossing from species to species, the connecting link which establishes the continuity of the chain. Apart from considerations of physiology only, the paleontologist sees no further occasion for debating the existence of connecting links or of passages from species to species, or as to how species originate. The mutation is the departure from the one, seeking adjustment and failing, or seeking and finding it in what must be recognized from accepted standards as a distinct specific form, a different species from its parentage. But when it comes to a matter of determining the rates, the time measure of these changes under varying and all conceivable physical conditions, the pursuit seems to us hopeless, hopeless a priori, hopeless in observation. There are species that have held their own without change through the ages—"immortal types" they have been called; and there are others which have yielded so rapidly to change that their evolution is explosive. The same facts are true of *groups* of animals; and for the entire organic world there have been earth-wide periods of long stagnation as well as of rapid intensive change. So long as an estimate of the age of the earth rests on evidence of the rate of change or adjustment in organisms through the acquisition of new characters, we may as well abandon the attempt to express it in concrete terms and satisfy ourselves that for the development of life the duration of that fraction of the earth's history is beyond human expression.

THE AGE OF THE EARTH FROM THE POINT OF VIEW OF ASTRONOMY.

By ERNEST W. BROWN.

(Read April 22, 1922.)

Astronomical evolution is considered under three heads: First, that method of observation in which it is assumed that all stages in the process are visible in the sky and so can be traced step by step. Second, physical theory, based on well-known laws such as those of gravitation, heat, etc. Third, pure speculation. When we attempt to apply these methods to the solar system, we find a complete absence of any observational evidence from the first point of view, because we have no stellar systems sufficiently near for us to detect planets if such exist. Thus evolution in the solar system is mainly a mixture of physical theory and speculation.

All theories of evolution use the idea of contraction under gravitation, which in general causes a gain of heat and of angular velocity. The chief differences between the theories consist in the forms of matter which are assumed to come into existence under the operation of the process of contraction. Laplace imagined that a planetary nebula contracted and in the course of the process left behind rings of matter which later condensed into planets. Roche showed that under certain conditions matter will be thrown off along the equator. G. H. Darwin and Poincaré developed the processes of fission from which it was hoped that planetary bodies might be shown to have developed through successive divisions of the central body. Later workers at the theory, and particularly Jeans, have proved that this hypothesis is very improbable for planetary evolution on account of the fact that in this process of division the masses should be of the same order of magnitude and not, as in the case of the planets, of very different orders of magnitude. It has, however, been applied with considerable success to the evolution of close double stars. Finally there are the tidal hypotheses in which the matter is supposed

to have been drawn off by the close approach of some second body which later moved away. Each of these hypotheses has many objections. But it may be stated that from these points of view we can learn nothing definite or even approximate about the age of the earth.

Another method of approach is through observation of the present condition of the bodies in the solar system. For evidence we have eight major planets, but it is very doubtful whether from so small a number we can deduce any results of value. In fact, it is now well known that differences in mass may produce very different consequences in the history of bodies. Thus arguments drawn from the Moon, Mars, Venus, or the other planets have never inspired very much confidence.

Still another method is a consideration of the present condition of the earth combined with the theory of contraction and subsequent loss of heat. Here we are on somewhat firmer ground, since we have many observations which give information concerning the interior condition of the earth. Amongst these may be mentioned the values of the mean density and the surface density, the phenomena of precession, nutation, etc., the measurements of earthquake and seismic waves, and measurements of the rigidity of the earth by various methods, and more particularly by that lately developed at Chicago by Michelson and his colleagues. From these phenomena we know with fair certainty that the earth *behaves* like a solid body which has approximately the rigidity of steel. It is sometimes assumed that this shows that the interior of the earth consists of matter which under surface conditions of pressure would be solid. Unfortunately the argument is doubtful, because we know nothing of the condition of matter under the pressures which it experiences at depths of one hundred miles or more below the surface of the earth. It is, therefore, impossible to argue with any security concerning the temperature conditions in the interior of the earth from these observational data. Lately Jeffreys has shown that under almost any theory of evolution the earth must at one time have been sufficiently hot so that all its materials were in a liquid state, understanding by this latter phrase, a state liquid under surface conditions of pressure.

Thus the astronomical evidence which can be furnished as to the

age of the earth is practically nil and one must turn to methods outside the range of the astronomer's work.

A further difficulty may be mentioned. Evidence is accumulating that there is widely extended diffuse matter in space, some of which is visible and some of which is only evident on account of the obscuration of light which it causes. It therefore seems highly probable that the solar system in the course of several hundred million years may have passed through one or several such clouds. These would have effects, which from theory are well known, such as diminishing the mean distances of the planets from the sun, the circularization of their orbits, possible changes in the total angular momentum of the system, and other effects such as the possible formation of comets and the production of glacial and interglacial periods. At present, however, the consequences of this hypothesis are still in the range of speculation and need to be worked out in considerable detail before any arguments can be built on it. It may, however, be stated that such a hypothesis would have the general tendency of increasing the age of the earth as estimated from other sources.

THE RADIO-ACTIVE POINT OF VIEW.

By WILLIAM DUANE.

In estimating the age of the earth one should measure the time that has elapsed by some process in nature that takes place in one direction only and that does not change its rate when conditions (temperature, pressure, etc.,) alter. In most of the estimates of geological periods of time that have been made, the "clocks" employed do not fulfill these conditions. Estimates based on the temperature of the earth, or of the sun, for instance, cannot be reliable, for the temperature of a body may fall or it may rise. Further, the rate of change of the temperature depends upon a variety of conditions, such as the amount of energy radiated, the supply of energy to it, etc.

Attempts have been made to deduce the age of certain minerals from the appearance of little round marks in them, called haloes. These haloes are supposed to be due to radiation from minute specks of radio-active matter at their centers. The colors produced by radiation in transparent substances depend, to a considerable extent, upon the temperature, so that no very great weight can be put upon geological periods of time estimated by means of haloes.

There are, however, other radio-active processes, the rates of which do not, so far as we know, depend on the temperature or the pressure, nor upon any other physical or chemical state.

During the last twenty-five years a large number of radio-active transformations of one chemical element into another have been discovered. Students of the subject agree that these transformations take place in one direction only, *i.e.*, from an element of higher atomic weight to an element of lower atomic weight. Further, nobody has been able to alter the rate of a radio-active transformation by any process whatsoever, although numerous attempts have been made to do so. These radio-active changes, therefore, seem to offer a reliable means of estimating certain periods of time.

Among the radio-active changes appear processes in which the metal uranium transforms itself through successions of intermediate stages into the metal lead and into the gas helium. It does not seem necessary to describe in detail these series of transformations at this time. Descriptions of them may be found in the literature on radio-activity. It suffices for our purposes to say that the rate of transformation is such that 5 per cent. of a quantity of uranium changes into lead and helium in about 370 millions of years.

We find uranium, lead and helium associated together in a great many minerals and it is natural to suppose that the helium and the lead were produced by the disintegration of the uranium during the past ages. Further, if we determine the relative amounts of uranium, lead and helium in a mineral we can form an estimate as to how long these chemical elements have been in contact with each other. Estimates of this kind that have been made from the quantities of helium in uranium ores vary between 8 and 700 millions of years, according to the locality from which the ore came. Since some of the helium (it being a gas) may have leaked out of the ores these intervals of time must be regarded as minimum estimates. The uranium and helium must have been in contact with each other for at least as long as the periods mentioned, but they may have been together for much longer intervals of time.

Calculations based on the quantity of lead in uranium ores vary from 340 millions to 1,700 millions of years, according to the locality from which the ore is obtained. In this case another complication appears. We have learned to distinguish several different kinds of lead from each other. The various kinds of lead have similar chemical properties but differ from each other in their atomic weights. All the different kinds of lead do not come from uranium; only lead of atomic weight about 206 may be regarded as produced from uranium. Until, therefore, we have determined exactly what the atomic weights of the lead in the various ores really are, we cannot be sure that the lead came from the uranium. We can assert, however, that there is no more uranium lead in a given uranium ore than the amount of lead actually found. Unless, therefore, the atomic weight of the lead in an ore has been actually determined and found to be about 206, we must consider the estimate of the age of the ore as a maxi-

mum estimate only. The lead and uranium cannot have been in contact with each other for a period of time longer than that calculated from the known rate of transformation of uranium into lead.

The atomic weight of the lead in a few ores has been found to be very close to 206. In one of these the age of the mineral has been estimated at a little over 900 millions of years.

The calculation of the age of uranium deposits by means of radio-active data rests upon the laws of nature as we now believe them to be. It would be a waste of time to speculate on future discoveries (new radio-active elements, for instance, or alterations in the rates of radio-active processes) or on a possible evolution of natural law.

The ages calculated from radio-active data represent the length of time during which we may suppose the chemical elements to have been in more or less mechanical contact with each other. They do not represent the time that has elapsed since the earth may have reached a state capable of supporting organic life as we now know it.

Harvard University Medical School,
Boston, Mass.

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"From the Palæontological Viewpoint," by John M. Clarke, Ph.D., Sc.D., LL.D., Director of the Department of Science and State Museum, Albany, N. Y.

"From the Astronomical Viewpoint," by Ernest W. Brown, M.A., Sc.D., Professor of Mathematics, Yale University.

"From the Radioactive Viewpoint," by William Duane, Professor of Biophysics, Harvard Medical School.

Special Meeting, May 5, 1922.

WILLIAM B. SCOTT, Sc.D., LL.D., President, in the Chair.

Hon. Roland S. Morris and Mr. Frederick E. Ives, newly elected members, subscribed the Laws and were admitted into the Society.

Letters accepting membership were received from:

Charles Elmer Allen, Ph.D., Madison, Wis.
Rollins Adams Emerson, Sc.D., LL.D., Ithaca.
Worthington C. Ford, A.M., Litt.D., Cambridge.
Frederick E. Ives, Philadelphia.
Roland S. Morris, A.B., LL.D., Philadelphia.
George W. Norris, A.B., M.D., Philadelphia.
Charles Lee Reese, Ph.D., Wilmington.
Robert DeC. Ward, A.M., Cambridge.

Prof. C. B. Bazzoni and Mr. Raymond Morgan, of the University of Pennsylvania, read a paper upon "Wireless Telephony and the Popular Use of the Radiophone in the Diffusion and Reception of Vocal and Instrumental Sounds, with Experiments," which was discussed by Prof. Snyder.

Stated Meeting, November 3, 1922.

WILLIAM B. SCOTT, Sc.D., LL.D., President, in the Chair.

The Patron, His Excellency the Governor of Pennsylvania, was present and took his seat on the platform.

Dr. Henry Skinner, a newly elected member, subscribed the Laws and was admitted into the Society.

Letters accepting membership were received from:

Irving Langmuir, Ph.D., Schenectady, N. Y.

Harlow Shapley, A.M., Ph.D., Cambridge.

Henry Skinner, M.D., Philadelphia.

James Perrin Smith, A.M., Ph.D., LL.D., Palo Alto, Calif.

Henry S. Washington, A.M., Ph.D., Washington.

David Locke Webster, A.B., Ph.D., Stanford Univ.

A letter was received from Dr. Charles C. Torrey, of New Haven, declining election.

The following letter from the Marquis Antonio de Gregorio was read:

PALERMO VIA MOLO 132.

Very Honored Sir:

I pray you to be so kind to insert in the PROCEEDINGS OF A. PHILOSOPH. SOCIETY this little paragraph on Gravitation and accept my best regards.

MARQUIS ANT. DE GREGORIO,
Cor. Member of A. Ph. Soc.

ON GRAVITATION'S KINETIC THEORY.

I have read with much interest the communication of Mr. Charles F. Brush on the Kinetic Theory of Gravitation (PROC. AMERIC. PHILOSOPH. SOC., Vol. LX., No. 2, p. 43). But I must observe that already in 1892 I proposed the same theory to the Academy of Sciences of Palerme. My work has the title "Sul Continuto della Spazio e sulla causa della gravitazione." It has been published in a volume "Nuovi Strumenti fisici e sulla piu probabile origine del nostro sistema stellare" (Palermo 1893) that has been published in the memoirs of our Academy of Sciences in Palerme. After many years, in 1914, I presented a work to the Societa Siciliano di Scienze Naturali upon the mere cosmogonic theories (Sulle nuove teorie cosmogoniche, sull' origine della materia). In this work, which has been published in the *Naturalista Siciliano* (April 1914) I have again discussed and elucidated and cleared this theory. I said that if there are two masses, they shall be pushed each toward the other by the difference of pression (tension) of ether, because the tension of ether is different in the space and in portion of it interposed between the two masses. So the two corps shall be converse each toward the other. I have considered ether as passing through the matter, but I observe that matter absorbs some part of its energy and transforms it: Radium phenomena are perhaps some of the manifestations of it. I considered matter not as a dead thing but as a living thing.

In 1921 M. Olinto De Pretto formulated a theory on gravitation that reproduced in part my theory, but he did not cite my work! He published a book "Lo spirito dell' Universo" (Turin 1921) in which he attributes gravitation to ether's action. In the review *Urania* that is published in Turin (x, n. 2, p. 75) there is a recension of it.

Gravitation is a most important natural phenomenon but hitherto unfortunately problematic. The work of Mr. Brush is valuable and commendable and his experiments are very instructive. But I think that my work is not to be neglected because it contains the priority of the theory and the foundation of the new conception of gravitation.

MARQUIS ANTONIO DE GREGORIO.

PALERMO, JULY 20, 1922.
SICILY, ITALY.

The decease was announced of the following members:

Alexander Graham Bell, LL.D., Ph.D., Sc.D., M.D., near Baddeck, N. S., on August 2, 1922, æt. 75.

Herman Diels, Ph.D., at Berlin, on June 4, 1922, æt. 74.

Merrill E. Gates, LL.D., at Littleton, N. H., on August 11, 1922, æt. 74.

Waterman T. Hewett, B.A., M.A., Ph.D., at London, England, on September 13, 1921, æt. 75.

Henry Marion Howe, A.M., LL.B., LL.D., Sc.D., at Bedford Hills, N. Y., on May 14, 1922, æt. 75.

Jacobus Cornelius Kapteyn, at Amsterdam, Holland, on June 18, 1922, æt. 72.

Alfred G. Mayer, M.E., Sc.D., in Florida, on June 25, 1922, æt. 54.

Joseph T. Rothrock, B.S., M.D., at West Chester, Pa., on June 2, 1922, æt. 83.

Coleman Sellers, Jr., M.S., at Bryn Mawr, Pa., on August 15, 1922, æt. 70.

Stephen Smith, M.D., at Elmira, N. Y., on August 26, 1922, æt. 99.

Edward Anthony Spitzka, M.D., at Mt. Vernon, N. Y., on September 6, 1922, æt. 46.

Ambrose B. Wyckoff, A.B., Lieut. U. S. N., at Ontario, California, on May 30, 1922, æt. 74.

Dr. John A. Miller read a paper on "The Rome Meeting of the International Astronomical Union," which was discussed by Prof. Snyder, Governor Sproul, President Scott, and Dr. W. W. Keen.

Stated Meeting, December 1, 1922.

William B. Scott, Sc.D., LL.D., President, in the Chair.

Mr. Charles Lee Reese, a newly elected member, subscribed the Laws and was admitted into the Society.

The following communication from Mr. Charles F. Brush, in reply to the letter of the Marquis de Gregorio, was read:

Through the courtesy of the Secretaries, it has been my privilege to read the letter of the Marquis de Gregorio and to offer such remarks as seem fitting.

Gregorio contributed a number of very interesting papers on many scientific subjects under the general title "Su Taluni Nuovi Strumenti," etc., published in the *Atti della Reale Accademia*, Palermo, 1895.

Professor Borgerhoff, of Western Reserve University, has very kindly prepared for me a careful translation of all of these papers, giving special attention to the last chapter, "On the Contents of Space and the Cause of Gravitation" (pp. 177–181).

I have studied this paper very carefully, and following is my understanding of its meaning: Gregorio assumes the ether to be everywhere in a state of "tension" (very evidently using this term in the sense of pressure) normally uniform. He says bodies of matter absorb ether, somewhat as spongy platinum absorbs gases, and the absorbed ether is rendered inactive or latent, analogous to latent heat. Different kinds of matter absorb different amounts of ether, and their densities depend on the amounts of ether absorbed. The free ether outside a body exerts pressure, not on the matter composing the body, but on the condensed or latent ether within it.

Absorption of ether by a body of matter reduces the "tension" (pressure) of the ether surrounding the body in proportion to the amount of ether absorbed. Two neighboring bodies will have less ether pressure against their parts facing each other than against their parts turned away from each other, and they will be urged toward each other by the superior ether pressure on the sides turned away.

Gregorio continues: "Instead of explaining the phenomenon" (gravitation) "by means of an absorption, one might also explain it

by means of a rotary molecular vibration. To this end we may suppose that ether is in a state of continual special vibration determining its tension, and that this vibration is decreased by its passage through the molecular interstices of the matter. The contrary might, however, be supposed, and this would appear more plausible, viz., that the phenomenon is caused by the vibration of the atoms composing the matter, and that the tension of the outside ether is transformed by the molecular vibration of the bodies. This seems to me the more plausible opinion."

In neither his ether-absorption theory nor his alternative hypotheses does Gregorio explain how the supposed diminution of ether pressure outside bodies of matter is maintained after absorption of ether has ceased: and indeed this seems to me impossible of explanation. Nor does he offer any suggestion of the source of the energy acquired by falling bodies.

My own "Kinetic Theory of Gravitation,"[1] put forth in December, 1910, is based on two fundamental concepts of the ether of space: First, that the ether is endowed with vast intrinsic energy—energy quite apart from matter—free energy. This was then and is now the belief of many eminent physicists. Second, that some, perhaps all, of the ether's intrinsic energy exists in wave form of some sort capable of motive action on matter, and propagated in every conceivable direction, so that the wave energy, or energy flux, is isotropic; and whereby a disturbance of any kind, anywhere in the ether, is in due time felt everywhere else, the intensity of disturbance diminishing with the inverse square of distance from its seat.

The last concept above I believe to be original with me. I find nothing of it in Gregorio's paper, but it is essential to my theory.

In his more recent paper, 1914, Gregorio reaffirms his 1892 theory of ether pressure and ether absorption by matter, and seeks to "develop my idea further" in the light of more recent discoveries in physics. He now regards the ether as a "dynamic fluid" composed of "imponderable particles, extremely minute, of the condensed fluid itself," which he calls "superatoms." He says further that "the actions of magnetic and electric currents" clearly prove "that an imponderable fluid can generate energy and, as it were, transform itself into energy." . . .

Again I find nothing of the second, and essential, postulate of my own theory.

Charles F. Brush.

Cleveland,
November, 1922.

[1] *Science*, March 10, 1911; *Nature*, March 23, 1911; Proc. Amer. Phil. Soc., Vol. LIII., No. 213, Jan.–May, 1914; Proc. Amer. Phil. Soc., Vol. LX., No. 2, 1921.

Prof. William Duane read a paper on "The Activities of the Radium and X-Ray Plants of the Cancer Commission of Harvard University," which was discussed by Doctors Keller, Keen, Goodspeed, and Reese.

The dates of the General Meeting of 1923 were fixed for April 19, 20, and 21.

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